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Pathways to the Future, Vol. 6
A Report on the National Longitudinal Surveys of Labor Market Experience of Youth in 1984

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EXECUTIVE SUMMARY

This volume of Pathways to the Future is the sixth report on the youth cohort of the National Longitudinal Surveys of Labor Market Experience, a nationally-representative sample of American young people who were age 14-21 when first interviewed in 1979. This report analyzes data from repeated interviews with these same individuals: 12,686 were interviewed in 1979; 12,141 in 1980; 12,195 in 1981; 12,123 in 1982; 12, 221 in 1983; and 12,069 in 1984. This data set records labor market activity in the unusual detail made possible only with annual reinterviewing; and a wide range of ability and knowledge test results, family background variables, and health and attitude measures are also available for each respondent. As with earlier volumes in this series, special attention is given not only to analysis of government-sponsored employment and training programs but also to a variety of other determinants of labor market success.

Chapter one examines welfare, education, and labor market outcomes for three groups--those who participated in government-sponsored employment and training programs in the period 1979-1981, those who were eligible but did not participate, and those who were not eligible. The possibility that CETA contributed to welfare dependency, perhaps by improving information about sources of support, should be considered, because CETA participants are more likely than others to receive welfare after they participate, taking into consideration prior welfare status. They also have a greater likelihood of staying on welfare if they are already receiving it at the time of participation, and a greater likelihood of moving onto welfare if they are not already receiving it. Income and weeks of unsubsidized employment are also lower for participants, although the income differential increases after participation while the weeks of employment differential decreases slightly. Wage rates are also slightly lower for participants, and they drop relative to eligibles following participation. No significant differences were found, however, in educational outcomes between those who had been in CETA programs and those who were eligible but not involved. Overall, CETA was not successful in improving labor market outcomes and reducing welfare dependency for these groups.

Chapter two aims to identify influences on the length of time young people are unemployed. Over a third of young men and over half of young women had spells of unemployment greater than a year--a finding that contradicts the common belief that most young people have frequent but short spells of unemployment as they move from job to job gathering labor market information. Significant determinants of nonwork periods for young men are age, expected earnings, unemployment income, living at home, being on temporary layoff, and living in a large metropolitan area. For young women the results are similar, except that marital status affects the length of nonwork periods and unemployment does not.

Chapter three examines a method for determining the existence and impact of selection bias, which is known to affect outcomes of labor policy discussions depending on how the bias is corrected. It finds that selection bias need not be corrected for when analyzing wage differences with these particular NLS data. It also finds that the usual corrections for selectivity bias would lead to much different, and in this case incorrect, conclusions about the determinants of wages.

Chapter four examines the long-run impact of teen-age unemployment on later labor market success. It finds that for those entering the labor market in the late 1970s and early 1980s, early unemployment did not reduce later wages except among minority women, and then only when their unemployment periods exceeded 24 weeks. For white male youth, as early unemployment increased, so did long-run earnings. The cost of early unemployment is concentrated in lost experience rather than in factors related to unemployment per se. Again contrary to the common belief about youth unemployment providing useful labor market knowledge, this positive relationship between early unemployment and later wages for white males could not be linked to productive search employment, in terms of job switching: young men who changed jobs while unemployed earned no more than those who didn't.

Chapter five examines variation within families in investment of psychic and material resources in their children's educational and occupational attainment process. The household sampling plan generated 5,863 respondents who had between one and five siblings who were also included in the survey. Of those in high school, significantly more parental encouragement went to the better academic performers and to those with the sibling status disadvantages of being younger or female, and this greater encouragement generated significant advantages to them for expected educational and occupational attainment. Among those in college, family financial assistance was greater for the college-attending siblings who were female and younger. These differences in assistance from families did not, however, affect the number of years of college attended. The findings suggest that the average American parent tries to compensate children who are likely to have difficulty by giving them more of their own resources, and at the same time tries to reward children who exhibit merit and a likelihood of success.

Chapter six adds evidence to other studies showing how pervasive employment is among high school youth: most youth work during their senior year of high school, and weekly hours exceeding 15 or 20 hours are common. Such employment has been shown to increase chances of finding jobs and to decrease unemployment after graduation. The current study finds that some youth who work as seniors have an easier school-to-work transition simply because they already have a post-school employer lined up and are spared the necessity of prolonged job search after graduation. These youth may have only deferred and not eliminated the need for job search if later they want to find positions offering greater career possibilities. The results reported here provide very little evidence that students who work acquire general or firm-specific job skills, that they are screened differently by employers, or that they learn useful job attitudes or behaviors. Because other studies have suggested that the increase in market work among high school students may account for declining national standards of academic excellence, more research into the outcomes of high school employment is needed.

Chapter seven provides information about the on-the-job training provided to young men by private employers, because it is this training that government-sponsored programs are designed to augment. Such information is crucial not only to government program design but also to understanding the components of labor market success. In the first years after formal schooling wage rates increase rapidly among male youth in all schooling groups, but wages differ widely within schooling groups; the differences are presumed to reflect a combination of luck, adaptation to the labor market, and on-the-job training. That job experience is valuable in developing marketable skills is attested to by 40 percent of the young men in the NLS sample. Furthermore, young men in high-learning jobs tend to stay in them, whereas half of those who in one year have no job, thus no learning, report no job in the next.

Those who agreed strongly that learning on the job increased their skills had earnings 13.5 percent higher than those who had previously been without work, and 7 percent higher than similar individuals in jobs that offered few if any learning opportunities. Individuals in higher learning jobs did not have lower wages during training, as might be expected, to equalize their later advantage. Given the magnitude of the on-the-job training effect on wage rates and the absence of equalizing differentials in current wages of jobs with high learning content, it would appear that on-the-job training contributes not only to differences in future annual earnings across individuals, but to differences in human wealth across individuals.

Chapter eight examines a sample of youth who left school between 1976 to 1978 and follows them for five to seven years after they left school. Youth who chose the all-volunteer force are included in sufficient numbers to be singled out as a comparison group. The different patterns of post-school work experiences by these youth are differentiated by sector of employment, weeks of work, and number of employers and occupations. The independent effect of each of these factors on wages is quantified for youth with civilian work experience between the 1982 and 1983 interview dates. Separate assessments are provided by sex and race, and a statistical adjustment is introduced in the wage equations to control for possible biases because some youth were not at work in that one year period. Findings indicate that decisions by youth about sector of employment, location, and other attributes of their job, and attachment to an employer affected their wages shortly after leaving school. Workers with comparable work experiences and backgrounds were not rewarded equally. Increases in the number of employer and occupation counts had no independent effect on wages.

The studies in this volume directly assess government policy and illuminate a number of issues that impinge on education and employment decisions. Understanding how labor markets work and observing the activities of individuals within these markets is crucial to the design and implementation of beneficial and effective government policies. As this volume demonstrates, ths NLS provides a unique source of such information.

Chapter 1

Participation in Government Employment and Training Programs and Patterns in Employment, Income, Education and Welfare

by

Richard Willke

I. INTRODUCTION

This chapter examines the impact of CETA programs on young people's transition from school to the labor market. Six years of panel data from the youth cohort of the NLS make it possible to select CETA participants or eligibles in years 1979, 1980, and 1981 and study their welfare recipiency, educational attainment and labor market outcomes for four years, including one year prior to the CETA year and two years following it. Several strategies to reduce heterogeneity between participants and eligibles as well as to account for selection bias and its effects are discussed and employed.

The effects of CETA on its participants have been widely and vigorously examined. Bassi (1983, 1984) examined the effect of CETA on labor market outcomes using the Continuous Longitudinal Manpower Survey and paid considerable attention to the problems of selection bias and sample heterogeneity. Her work involved an older group of individuals than is present here, however, and some of her methods rely on longer employment histories than are available in the NLS youth cohort. The question of how correlations in earnings outcomes over time should be handled is the subject of recent work by Bloom (1984). Kiefer (1979) discusses necessary considerations when longitudinal data are used to estimate the effects of training programs. The present chapter incorporates many of these considerations in evaluating outcomes and in formulating a specific model to estimate CETA effects.

Crowley (1984) investigated high school graduation, welfare status, and earnings outcomes in 1981 as a result of CETA participation during the previous three years. Using an underlying model similar to the one here, and employing five years of youth cohort data, Willke (1984) examined the effects of participation on changes in employment outcomes. Using the first three years of NLS data, Hahn and Lerman (1983) investigated the question of whether CETA worked for disadvantaged youth. Their findings on CETA are somewhat similar to those found here.

The next two sections discuss sample design considerations and sample selection. Sections four and five examine welfare dependency and educational attainment, and section six the labor market outcomes of earned income, weeks of unsubsidized employment and wage rates. Section seven estimates a pooled-time series cross section model and section eight a selection bias model. A final section summarizes the results and draws conclusions.

II. SAMPLE DESIGN

The members of the NLS cohort participated in a wide range of CETA programs--at least 23 different subsidized employment programs and eight subsidized training programs. Although 2,625 NLS respondents participated in a CETA program between 1978 and 1983, evaluating the impacts of each separate program would lead to some very small sample sizes and complex questions about the appropriate comparison group in each case, for example, Exactly which people are eligible for which program? and Should participants in one program be compared to participants in another program or only to non-participants?

This study will focus only on a generic "CETA participation" category, with some differentiation of summer versus non-summer programs and employment versus training programs. Other studies have evaluated individual CETA programs, using samples specifically chosen for their purposes.[1] The NLS is better designed for a more general evaluation, in which those choosing among the many options available under CETA are compared to those not involved in any CETA program.

The choice of a comparison group is crucial. Although it is reasonably straightforward to document the labor market outcomes of people after their CETA experience, any attempt to classify those outcomes as good or bad would immediately prompt the question "As compared to what?" Such outcomes can only be evaluated relative to outcomes that would have occurred without the CETA "treatment." However, since CETA participants may not be randomly chosen from an eligible population and we cannot observe the same person not in CETA, a perfect control group is not possible.

The usual approach is to match a set of eligible non-participants to the participant group, thus providing a set of similar individuals whose labor market outcomes might be expected to be similar to those of CETA participants had the latter not chosen to participate. Because not all characteristics of individuals are known, however, unobserved differences may affect outcomes.

If those unobserved differences have unchanging effects on outcomes, i.e., the person with less "ambition" earns $1,000 less each year, all else constant, the appropriate outcomes to study are individual changes in earnings, employment, etc., from before CETA participation to after CETA participation. The non-participant comparison group then provides a benchmark for what a "normal" change is. This approach has well-recognized merit and is widely used, but it limits this study because many NLS respondents are in their mid- to late teens during the "before CETA" period, and during the teen years labor market experiences and choices are much more variable than later in life. Thus, the decision to participate in CETA may be less related to fixed unobserved difference, such as motivation, than to short-term unobserved differences, such as an accident. Nevertheless, this approach is one of the principal strategies used here, namely, explicit control for outcomes observed before or during CETA participation.

As noted, it is also probable that there are temporary unobserved differences between participants and non-participants, especially in the period leading up to CETA participation. These differences, be they poor job matches, poor labor market information, personal problems or simply bad luck, may account to a large degree for the relatively poor labor market outcomes commonly observed prior to participation.[2] These differences may indeed lead to selection of (or selection for) CETA, and thus cause selection bias.

To the extent that these temporary unobserved differences induce participation in CETA and would normally be reduced or disappear with or without CETA, the changes attributed to CETA can be overstated because the prior period measurement is abnormally low. This problem has been discussed and documented in more detail for the NLS youth cohort CETA participants in Willke (1984). Without some correction, observation and analysis of changes in outcomes can exacerbate bias due to temporary effects while reducing the fixed effect problem. On the other hand, analysis of post-CETA outcome levels is not affected by pre-CETA temporary differences which have subsided, but is still plagued by fixed effects. These factors must be kept in mind when examining levels or changes. We must specify more structure to properly examine this problem. Some specification is given below and in Appendix 1A.2.

CETA participation provides permanent source of human capital to improve subsequent market outcomes, and it is also designed to reduce or eliminate some of the temporary problems that lead to participation.[3] This feature changes the way in which selection bias should be interpreted, and partially reduces the selection bias objection to analyzing changes in outcomes.

No method of analysis is without potential bias. In order to minimize bias two separate study samples have been selected. These serve to qualify analysis of both outcome levels after CETA participation as well as outcome changes from pre-CETA to post-CETA years. Statistical corrections will be applied to reduce both the fixed effect and the selection bias problems.

III. SAMPLE SELECTION

Both samples were constructed using a four-year spell structure consisting of a focal year in which CETA participation or eligibility is established, the year previous, and two years subsequent to the focal year. The NLS now contains data for 1978 through 1983, so focal years 1979, 1980, and 1981 each allow for a complete spell of data. For consistency and comparability, and to avoid incomplete spells, no focal years outside this range are used here. Analysis of various outcome levels and changes are completely comparable because they can be performed for everyone in the sample.

In each year the family and/or individual income of a respondent allowed determination of whether the respondent was below either the OMB poverty level or the Lower Living Standard Income Level for the area, making them eligible for many CETA programs. Although CETA eligibility determinations are based on income from a period shorter than a year, many income figures in the NLS are available only on a yearly basis, so our determinations relied on these.[4] Some CETA programs probably pick up much eligibility that the yearly income criteria miss, for example, those that are available to those who are recently unemployed or separated from the military. These factors were also taken into account, and a resonable approximation of CETA eligibility is created for each year.

The first sample, to be called the edited sample, contains one four-year spell for each individual who either participated or was eligible for CETA in one of the focal years 1979, 1980, or 1981. The limitation to eligibles as the comparison group should reduce the unobserved differences between participants and non-participants because both are experiencing problems leading to eligibility. Of course, differences leading to actual participation may still be present, and may be either fixed or temporary. These specifications led to a male sample of 777 participants and 3217 eligible non-participants, and a female sample of 750 participants and 3449 eligibles. These groups are described more precisely in Appendix 1A.1.

The edited sample is similar to most samples constructed for CETA and other evaluation purposes. However, permitting only one spell per person allows only one participation or eligibility year as the focal year, whereas many individuals participate or are eligible in more than one year. Although the participation year is picked using the year of longest participation, and eligible years are picked to maximize their similarity to participation years, this process nevertheless reduces the use of available information on CETA involvement.[5] It also relies heavily on the eligibility determination in the focal year, to reduce fixed effect differences between the groups in all four years.

An alternative used here, termed the unedited sample, includes each possible four-year spell for every respondent, and produces a sample nearly three times as large as the number of respondents.[6] The spells are classified according to the status of the respondent during the focal year, i.e., as a participant, eligible or non-eligible spell. In this way an individual may contribute a combination of participant, eligible, and non-eligible spells to the sample, possibly making the group more comparable in unobserved characteristics, particularly since many participants are eligible in other years. This sample design leaves CETA participation as the more principal distinction between groups. In addition, repeat participants or eligibles are weighted more heavily in their respective groups, a characteristic which may be seen as capturing the "real" members of a particular group. On the other hand, if repeat participants have "stronger" fixed effects than others, the fixed effect is not particularly alleviated, and may be exaggerated. This procedure created a male sample of 951 participants with 5509 eligible and 6959 non-eligible spells, and a female sample of 947 participants who had 5955 eligible and 6698 non-eligible spells.

The unedited sample structure is less common than the edited sample in training program evaluation, but analysis of spell data with multiple spells per individual is becoming common in other domains of labor economics. Both samples have advantages and disadvantages which sometimes provide useful contrasts, but neither one addresses the selection bias problem significantly better than the other.

In the following two sections, welfare recipiency and educational attainment will be examined using both level and change data but without further statistical adjustment for selection bias. Earnings and employment outcomes are next studied using a graduation of techniques to address such problems, and these will be discussed as they are used.

IV. WELFARE DEPENDENCY

Two types of analysis shed some light on the welfare recipiency status of CETA participants versus eligibles, non-participants and non-eligibles. Welfare recipiency is defined as having received any payments for AFDC, SSI, general assistance, or food stamps during a given year. A strictly tabular analysis is presented for both the edited and unedited sample groups. This analysis shows on-welfare off-welfare percentages and transitions controlling for sex, CETA status, and prior welfare status. Although these are the most interesting controlling characteristics, many others may influence welfare recipiency. A multivariate probit analysis to explain welfare status in the second year after participation or eligibility provides further evidence on CETA-welfare correlations.

Table 1.1 presents, for each sex and sample, the percent of each eligibility group that is on welfare in the focal year and two years later. Here, as always, eligibility group status refers to CETA participation or eligibility in the focal year only. It also presents an entrance rate, which is the percentage of people not on welfare in the focal year who are receiving welfare two years later, and an exit rate, which is the percentage of people receiving welfare in the focal year who are not on welfare two years later.

Uniformly across sexes and samples, participants are more likely to be on welfare in each year and are more likely to enter and less likely to exit, compared to eligibles and non-eligibles. This finding reveals some basic differences between the groups not accounted for by the eligibility determination (although eligibles are more comparable to participants than to non-participants). The welfare percentages of participants and eligibles are somewhat closer two years later, but the differences are still statistically significant, except among females in the unedited sample. The differences in

Table 1.1 Welfare Status and Transitions, by Sex, Sample and Eligibility Category

	On Welfare				
	Focal Year	2 Years Later	Entrance Rate[a]	Exit Rate[b]	Total Sample
			Males		
Edited Sample					
Participants	28.6	18.0	8.7	58.6	554
Eligible Non-Participants	15.1	9.6	4.7	63.0	3214
Unedited Sample					
Participants	27.3	19.8	10.0	54.2	951
Eligible Non-Participants	18.6	11.3	5.5	63.0	5506
CETA Ineligible	0.0	2.3	2.3	--	6959
			Females		
Edited Sample					
Participants	29.8	23.9	11.6	47.1	749
Eligible Non-Participants	20.0	16.0	7.3	49.2	3448
Unedited Sample					
Participants	28.7	24.6	13.4	47.6	944
Eligible Non-Participants	25.5	21.1	9.2	44.1	5955
CETA Ineligibles	0.0	4.3	4.3	--	6698

[a] Percentage of people not on welfare in focal year receiving welfare two years later.

[b] Percentage of people receiving welfare in focal year not receiving welfare two years later.

all the entrance rates and some of the exit rates are also statistically significant. These differences, however, may be due to differences in marital status, children, etc., which are not accounted for here. Before speculating on unobservable reasons for this heterogeneity, we shall examine observable reasons using the probit analysis.

Table 1.2 presents coefficient estimates for probit analyses which explain whether an individual was a welfare recipient two years after the focal year. The signs of these coefficients indicate the direction of change in probability of being on welfare due to the given explanatory characteristic. Welfare recipiency in the first two years is controlled for, and it is a strong positive predictor of later welfare status, as expected. The use of both years' status was intended as a control for welfare history and dependency.[6] However, many other variables significantly increase or reduce the probability of welfare recipiency. Notably, being a CETA participant has a positive effect on that probability in all cases, and is statistically significant in three out of four cases. Summer participation in CETA does not affect the probability of welfare in any significant way, but subsidized employment in CETA reduces the effect for males in the edited sample and increases it for females in the unedited sample.

Other control variables are age, number of children, marital status, highest grade completed, weeks not working (all measured in the focal year), whether the family's income was below the OMB poverty level two years after the focal year, AFQT score, and minority status. For the unedited sample, CETA eligibility is also controlled. These variables generally have expected and consistent effects, and cover most major reasons for being on welfare. Two of these variables in particular--OMB poverty status and weeks not working--control for characteristics which together might be thought to

Table 1.2 Probit Estimates for Welfare Recipiency Two Years Later, by Sex and Sample

(t-statistic in parentheses)

	Males		Females	
Variable	Edited Sample	Unedited Sample	Edited Sample	Unedited Sample
Welfare Recip., Prior to Focal Year	.570 (7.61)**	.590 (11.7)**	.364 (5.34)**	.479 (10.9)**
Welfare Recip., Focal Year	.902 (12.7)**	.846 (16.1)**	.898 (13.8)**	.836 (18.0)**
CETA Participant	.369 (2.50)*	.160 (2.20)*	.167 (1.12)	.176 (2.46)**
CETA Subsidized employment	-.339 (-2.06)*	-.141 (-0.97)	-.006 (-0.03)	.269 (1.88)
CETA summer participant	.031 (0.23)	.005 (0.03)	.053 (0.41)	-.080 (-0.67)
CETA Eligible	--	.145 (2.95)**	--	.157 (3.68)**
Age	.0017 (0.10)	-.0002 (-0.02)	-.0469 (-2.99)**	.0061 (0.68)
Children	.151 (2.26)*	.170 (4.35)**	.456 (12.4)**	.362 (16.3)**
Years of School	-.0567 (-3.23)**	-.0627 (-6.01)**	-.0191 (-1.11)	-.0533 (-5.46)**
Married, Spouse Present	-.248 (-2.53)*	-.182 (-3.30)**	-.736 (-10.2)**	-.797 (-18.5)**
AFQT Score	-.0032 (-2.03)*	-.0038 (-3.96)**	-.0050 (-3.15)**	-.0033 (-3.45)**
Below OMB Poverty Level	.409 (6.40)**	.365 (8.94)**	.322 (5.68)**	.353 (9.85)**
Weeks Not Working, Focal Year	.0042 (2.30)*	.0058 (5.17)**	.0053 (3.13)**	.0099 (10.3)**
Black/Hispanic	.098 (1.47)	.072 (1.68)	.050 (0.95)	.161 (4.20)**

Table 1.2 (continued)

Variable	Males Edited Sample	Males Unedited Sample	Females Edited Sample	Females Unedited Sample
Intercept	-1.166 (-3.04)**	-1.039 (-3.57)**	-.310 (-0.85)**	-1.606 (-5.89)**
X^2(d.f.)	714.4 (13)**	1883.6 (14)**	1282.4 (13)**	3955.0 (14)**

** Statistically significant at the .01 level.
* Statistically significant at the .05 level.

represent the people who are consistently poor and not very attached to the labor market; in other words, people who are likely to be repeated welfare recipients. The fact that CETA participation is still a significant explanatory factor suggests further, subtler differences. Two of many possible explanations for this correlation are: first, that CETA participation represents some pre-existing heterogeneity, be it generally lower income or labor force attachment, or greater information about or willingness to take advantage of government programs, despite variables which at least partially control for these characteristics; second, that CETA contributes to welfare dependency, possibly through making participants aware, via contact with other participants, of the types of government support available. Without more precise ways to control for all characteristics explaining welfare dependency, it is difficult to choose between these explanations. In any case, it is difficult to claim, based on the evidence here, that CETA participation reduces welfare dependency.

V. EDUCATIONAL ATTAINMENT

For purposes of taking an introductory look at CETA's effect on education and gauging the comparability of eligible and participant groups, it is useful to view some cross-tabular statistics on educational attainment and additional schooling completed for the various groups. Schooling is ongoing during the sample period, so age is controlled in examining schooling levels. Schooling attained prior to the focal year is the principal control factor in examing additional schooling.

Table 1.3 presents average years of schooling completed in the last year of the spell by age, sex, eligibility status and sample group. For example, the average male CETA participant in the edited sample who was 20 years old in

Table 1.3 Average Years of Schooling Completed Two Years After the Focal Year, by Sex, Age, Sample, and Eligibility Status

	Edited Sample		Unedited Sample		
	Participants	Eligibles	Participants	Eligibles	Non-Eligibles
			Males		
Age Two Years After Focal Year					
17	10.75*	10.33*	10.75*	10.00*	10.64
18	11.00*	10.87	11.42	10.92	11.25
19	10.94	11.08	10.81	11.08	11.83
20	11.39	11.53	11.44	11.37	12.17
21	11.55	11.77	11.55	11.57	12.28
22	11.89	12.02	11.65	11.77	12.37
23	12.27	12.36	12.16	12.00	12.47
24	12.14	12.56	12.20	12.29	12.60
25	11.81	12.71	11.97	12.45	12.93
26	12.00	12.60	12.39	12.43	13.20
27	-- *	13.63*	16.00*	12.43	13.01
All	11.52	11.90	11.62	11.87	12.48
			Females		
Age Two Years After Focal Year					
17	10.00*	10.00*	10.00*	10.00*	10.72
18	11.00*	10.71	11.13*	10.79	11.44
19	11.66	11.31	11.60	11.35	11.90
20	12.24	11.74	12.10	11.63	12.30
21	12.04	12.11	12.05	11.87	12.50
22	12.40	12.27	12.28	12.05	12.61
23	12.49	12.40	12.47	12.25	12.69
24	12.23	12.59	12.48	12.42	12.75
25	12.73	12.81	12.55	12.39	12.99
26	12.69	12.90	12.65	12.29	13.14
27	16.00*	14.00*	14.50*	12.34	13.17
All	12.15	12.07	12.18	12.07	12.63

* Less than 20 observations in the cell.

the last year of the spell--two years after the focal year of participation or eligibility--had completed 11.39 years of schooling by the last year of the spell. For males in the edited sample, educational attainment for participants is slightly lower than for eligible non-participants, but the differences are not statistically significant until age 24 and above. For males in the unedited sample, the differences between eligibles and participants do not show any significant pattern by age. The lower average across all ages reflects the fact that participants are slightly younger, but within age groups educational attainment is fairly equal. This result probably is due to the blurring of fixed differences caused by our sampling technique. However, non-eligibles have significantly more education at all ages, showing that despite any crossover between groups, the differences are still sharp.

Female eligibles and participants have very comparable schooling in the edited sample. In the unedited sample, female participants have consistently more education by age than eligibles, although the differences are not generally statistically significant. Female non-eligibles have higher levels of schooling than both groups.

As discussed earlier, the levels of schooling attained by various eligibility groups could be influenced by different starting points or fixed effects for each group, and thus may show a different correlation with CETA participation and eligibility than do changes in schooling attained during the spell. However, examination of Table 1.4, which shows average additional years of schooling completed between the first and last years of the spell as well as whether or not the individual spends any time at all in school during the first year of the spell, reveals the same basic patterns as in the levels data. Male participants obtain slightly less additional schooling than their

Table 1.4 Average Additional Years of Schooling Completed During Four-Year Spell, by Sex, Sample, Eligibility Status, Initial Schooling, and Initial Enrollment Status

	Edited Sample		Unedited Sample		
	Participants	Eligibles	Participants	Eligibles	Non-Eligibles
			Males		
Not in School, Year 1					
Years of Schooling, Year 1					
0-7 years	0.00*	0.09	0.63	0.10	0.10
8 years	0.00	0.05	0.00	0.07	0.18
9-11 years	0.11	0.15	0.15	0.14	0.20
12 years	0.11	0.13	0.09	0.13	0.08
13-15 years	0.00*	0.19	0.00*	0.13	0.07
16 years	-- *	0.40*	0.00*	0.40*	0.18
In School, Year 1					
Years of Schooling, Year 1					
0-7 years	1.39*	1.75	1.44*	1.33	1.91
8 years	1.55	1.58	1.35	1.50	2.28
9-11 years	1.56	1.69	1.45	1.36	1.78
12 years	0.72	0.86	0.80	0.84	0.85
13-15 years	1.32	1.42	1.39	1.33	1.32
16 years	1.00	0.59	0.57*	0.91	0.58
			Females		
Not in School, Year 1					
Years of Schooling, Year 1					
0-7 years	0.40*	0.35	0.33*	0.29	0.08
8 years	0.36*	0.06	0.50*	0.06	0.11
9-11 years	0.24	0.08	0.27	0.09	0.15
12 years	0.18	0.09	0.21	0.08	0.07
13-15 years	0.00*	0.11	0.00*	0.08	0.10
16 years	-- *	0.00*	-- *	0.14*	0.07
In School, Year 1					
Years of Schooling, Year 1					
0-7 years	0.00*	0.91	-- *	1.00	1.43
8 years	2.13*	1.55	1.60*	1.73	2.59
9-11 years	1.78	1.76	1.63	1.45	1.97
12 years	0.98	0.94	0.95	0.85	0.89
13-15 years	1.25	1.33	1.20	1.26	1.13
16 years	0.00*	0.43	0.00*	0.42	0.27

* Less than 20 observations in the cell.

eligible counterparts in the edited sample, but the difference is small, and does not depend on initial enrollment status. However, although out-of-school female participants in this sample go back to school more than eligibles, in-school participants and eligibles are very comparable.

In the unedited sample, male participants and eligibles, and in many categories, non-eligibles, have very similar records of additional schooling, although non-eligibles who haven't finished high school are distinctly more likely to do so. Once again, out-of-school female participants seem more likely to go back to school than either eligibles or non-eligibles, but if in school, especially high school, participants have a slight advantage over eligibles, and non-eligibles are much more likely to continue their schooling.

In summary, this evidence on school attainment finds male CETA participants to be very close to or just slightly below the par set by CETA eligible non-participants, except at older ages, where the differences in levels is somewhat more significant. Female participants, on the other hand, are very close to or slightly above the par set by eligibles, with greater differences in their likelihood of going back to school if they are not in school during the first year of the spell. Non-eligibles of both sexes pursue more schooling than eligibles or participants, particularly when it comes to finishing high school.

It is difficult to draw a definite conclusion from these results except by comparing them with the findings in the welfare section. If sample heterogeneity was driving the welfare results, it does not seem to apply to education, unless CETA participation has a strong positive effect on education. This conclusion is not supported by the fact that, with a few exceptions, both schooling changes and end levels were comparable for eligibles and participants, implying that initial levels were similar as well and showing no signs of marked heterogeneity. If educational homogeneity implies general homogeneity, then CETA must induce welfare recipiency. The more likely conclusion is that heterogeneity or other bias affecting welfare outcomes is different from that affecting education outcomes. Finally, it is possible that since education is usually government-provided, a CETA participant characteristic of taking advantage of government programs could be at work here as well.

VI. LABOR MARKET OUTCOMES

This section focuses on labor market activity during the four year spell. The particular outcomes of interest are earned income, weeks of unsubsidized employment, and the highest hourly wage rate for each individual during each of the four years of the spell. These outcomes will be the subject of a series of efforts to isolate a CETA participation effect from the fixed-effect heterogeneity and selection bias that may contaminate it.

Tables 1.5a and 1.5b display means for each year for earned income, weeks of unsubsidized employment, highest hourly wage, weeks unemployed, and weeks not working for any reason. These tables make it clear that participants are consistently worse off than eligibles--they have lower wage rates and less earned income in all years, and are unemployed or not working more often in all years but the focal year. Non-eligibles are better off in all categories. Clearly there is some heterogeneity at work here, but some of the differences could be due to easily identifiable human capital characteristics, such as age, education or current time spent in school. These characteristics will be controlled for later.

Consideration of average changes in levels reveals a much closer comparability between eligibles and participants. For instance, the average

Table 1.5a Average Values for Labor Market Outcomes During the Four-Year Spell, *by Sample and Eligibility Status: Males*

	Edited Sample		Unedited Sample		
	Participant	Eligibles	Participants	Eligibles	Non-Eligibles
Earned Income[a]					
Year 1	$ 2435	$ 4599	$ 2794	$ 4689	$ 7207
Year 2	3328	4708	3500	4912	9049
Year 3	4081	5958	4274	6142	9687
Year 4	5186	7319	5362	7309	10623
Weeks of Unsubsidized Employment[a]					
Year 1	13.9	21.4	13.4	22.0	30.5
Year 2	11.0	22.8	12.7	23.7	34.5
Year 3	22.1	28.1	20.6	27.8	34.0
Year 4	25.3	31.0	24.3	30.4	35.4
Highest Hourly Wage[a,b]					
Year 1	$ 4.97	$ 5.79	$ 5.04	$ 5.80	$ 6.44
Year 2	4.98	5.92	5.07	5.98	6.82
Year 3	5.11	6.05	5.26	6.02	7.04
Year 4	5.65	6.21	5.70	6.18	7.28
Weeks Unemployed					
Year 1	8.9	4.7	8.4	7.4	2.2
Year 2	8.2	9.5	9.1	11.1	0.3
Year 3	10.3	8.1	10.3	9.5	2.9
Year 4	10.4	7.2	10.3	8.3	3.8
Weeks Not Working					
Year 1	29.7	23.3	26.7	22.8	13.2
Year 2	22.5	24.2	23.1	23.8	9.0
Year 3	24.2	20.1	23.6	20.0	10.3
Year 4	22.1	16.4	21.9	16.8	10.1

[a] Expressed in 1983 dollars
[b] Only for those with positive wages

Table 1.5b Average Values for Labor Market Outcomes during the Four-Year Spell, by Sample and Eligibility Status: Females

	Edited Sample		Unedited Sample		
	Participant	Eligibles	Participants	Eligibles	Non-Eligibles
Earned Income:[a]					
Year 1	$ 1608	$ 2513	$ 1978	$ 2630	$ 4872
Year 2	2458	2738	2675	2741	6096
Year 3	2844	3746	3197	3692	6444
Year 4	3603	4660	3816	4537	6980
Weeks of Unsubsidized Employment					
Year 1	11.2	17.9	10.7	18.0	28.6
Year 2	9.8	19.1	12.0	19.4	32.6
Year 3	20.8	23.9	20.8	22.9	31.9
Year 4	24.3	26.1	24.8	25.2	32.7
Highest Hourly Wage[a,b]					
Year 1	$ 4.50	$ 4.73	$ 4.46	4.72	$ 5.32
Year 2	4.50	4.83	4.59	4.77	5.55
Year 3	4.51	4.93	4.60	4.92	5.63
Year 4	4.76	5.13	4.79	5.11	5.80
Weeks Unemployed					
Year 1	6.9	3.8	6.1	5.6	2.1
Year 2	6.5	7.3	6.1	8.1	0.3
Year 3	7.4	5.8	6.9	6.2	2.2
Year 4	6.5	4.8	6.4	5.3	2.8
Weeks Not Working					
Year 1	33.1	30.2	30.4	29.8	18.9
Year 2	25.1	30.1	25.0	30.4	15.0
Year 3	27.3	26.1	25.6	27.1	16.2
Year 4	26.3	24.1	25.3	25.1	16.0

[a] Expressed in 1983 dollars
[b] Only for those with positive wages

difference in earned income between year one and year four for males in the edited sample is $2,751 for participants and $2,770 for eligibles--essentially identical. Many of the comparisons are nearly as close, and the participants improve slightly more than the eligibles as often as not. Ignoring for the moment that there are many observable factors that should be controlled for, if changes in levels cancels out much of fixed heterogeneity between the groups, then resulting similarity in improvements would lead to a preliminary conclusion that CETA has no more or less effect than other options chosen by non-participating eligibles. However, a selection bias argument such as that of Silkman, Kelley and Wolf (1983) would argue that participants chose CETA because they were having an abnormally bad year. Since luck would persist with or without CETA, one might expect participants to improve more than non-participants if they experienced normal luck in succeeding years. In this case, CETA participation would appear to have slowed the improvement of the participants since their improvement was no better than others'. Hence the selection bias argument imposes somewhat more stringent requirements on the improvement of participants in order for CETA to take any credit.

However, since part of the role of CETA is to pull people out of labor market difficulties, part of its effect might be a temporary one that reverses the individual's luck faster than usual. This effect by its nature would become smaller as time progressed, and could thus be hidden by a selection bias correction. However, CETA should be given credit for its temporary as well as its permanent effects.

Uncovering these temporary and permanent effects is a subtle problem, and it has not been approached this way before. The stages of analysis in this section are meant to explain the improvement of CETA participants and eligibles in a step-by-step manner that reveals the selection bias effect, its correction, and the intermingling of the selection bias effect with the temporary CETA effect.

From this point, only the edited sample will be used. It has become obvious that the labor force experiences of non-eligibles are significantly different from those of eligibles and participants, thus they may not constitute a useful control group. In order to perform regression analysis, we need to assume that the equation structure and control variable coefficients are similar across groups in order to get a useful measure of the CETA effect. Since the non-eligibles are different in other respects, they are not likely to have the necessary similarity of structure. In addition, the fact that the data contain multiple spells for individuals clashes with some of assumptions necessary if we are to use econometric techniques that are appropriate here.

VII. POOLED TIME-SERIES CROSS-SECTION RANDOM COMPONENTS MODEL ESTIMATION

To establish a basis for comparison with later results, our first estimation results presented are produced by a pooled cross-section time-series random components model. This technique accounts for individual- and time-specific components in the outcome equation, utilizing the four available observations per individual to correct for a certain amount of individual heterogeneity. Although outcome levels rather than changes are estimated, permanent unobservable effects on outcomes are efffectively controlled for. The coefficients of the explanatory variables are assumed to be constant for all four periods, except for the CETA-related variables, which are interacted with dummy variables representing the appropriate time series unit. Selection bias is not controlled for in any deliberate way. The well-known hazards to selection bias corrections make base-line estimates desirable. The presence of selection bias relating to participants will be indicated by a significant coefficient in the CETA participation dummy in the period prior to the focal years--the first year of the spell--in which, having controlled for permanent individual effects, the CETA variable should not be significant if no selection bias is present.

Table 1.6 presents estimated coefficients and t-statistics for CETA variables from the random components model. Each particular equation was estimated twice, once with only a single CETA participation variable per year and once with addition controls for subsidized employment programs and summer programs. Other variables in the model are age, age squared, marital status, children, education, AFQT score, minority status, region, and weeks in school. All members of the edited samples are included in the income and employment regressions, while only those with wages in all four years can be included in the wage models.

These estimates show some clear differences not otherwise accounted for between CETA participants and eligibles. Considering Model 1 estimates, we see that male participants have higher earnings but lower wages than eligibles in the year prior to the focal year. This observation would seem to be inconsistent with the 5.8 fewer weeks of unsubsidized employment in that year which participants exhibit (see Table 1.5a). However, many participants also have more weeks of subsidized employment in the prior year and work more total hours, thus accounting for higher income despite lower wage rates. Although part of the reason for subsidized employment in the prior year is simply overlap of the same program spell which drove selection of the focal year, it is also true that much of it comes from other program spells. For example, of the 2,625 members of the NLS sample who participated in a CETA program at any time during the six years of the survey, 40 percent had more than one spell of participation.

Table 1.6 CETA Impact Coefficients in Random Components Labor Market Models

(t-statistics in parentheses)

Dependent Variable	Model 1	Model 2		
	Participation	Participation	Subsidized Employment	Summer
	Males			
Earned Income (log)				
Year 1	.228 (2.12)*	-.499 (-1.92)*	1.08 (3.82)**	-.677 (-3.02)**
Year 2	.983 (9.29)**	-.481 (-1.85)*	.531 (1.55)	.322 (1.44)
Year 3	-.194 (-1.85)*	-.916 (-3.52)**	-.592 (-1.73)*	-.536 (-2.39)*
Year 4	-.314 (-2.96)**	-.589 (-2.26)*	-.680 (-1.99)*	-.077 (-0.35)
Weeks Unsubsidized Employment				
Year 1	-5.82 (-7.57)**	-7.76 (-4.28)**	2.01 (1.03)	0.82 (0.53)
Year 2	-10.22 (-13.3)**	-12.82 (-7.06)**	0.63 (0.30)	1.23 (0.80)
Year 3	-4.50 (-5.85)**	-3.04 (-1.67)*	-3.63 (-1.75)*	-2.22 (-1.43)
Year 4	-4.33 (-5.63)**	-4.18 (-2.30)*	1.44 (0.69)	-1.96 (-1.26)
Highest Wage				
Year 1	-.59 (-2.94)**	-.63 (-1.14)	-.033 (-0.06)	.327 (0.76)
Year 2	-.52 (-2.65)**	-.76 (-1.36)	.298 (0.44)	.018 (0.04)
Year 3	-.67 (-3.44)**	-.45 (-0.81)	-.443 (-0.65)	-.254 (-0.59)
Year 4	-.78 (-4.02)**	-1.01 (-1.82)*	.608 (0.89)	-.667 (-1.54)

Table 1.6 (cont.)

Dependent Variable	Model 1	Model 2		
	Participation	Participation	Subsidized Employment	Summer
	Females			
Earned Income				
Year 1	.662	-.361	1.53	-.843
	(5.15)**	(-1.20)	(4.66)**	(-3.26)**
Year 2	1.67	.483	-.193	.210
	(13.0)**	(1.61)	(-0.51)	(0.81)
Year 3	.357	-.223	-.501	-.424
	(2.79)**	(-0.74)	(-1.31)	(-1.64)
Year 4	.131	-.397	-.151	-.144
	(1.02)	(-1.32)	(0.69)	(-0.56)
Weeks Unsubsidized Employment				
Year 1	-5.01	-8.09	3.75	-0.24
	(-6.82)**	(-4.74)**	(2.02)*	(0.316
Year 2	-8.20	-11.24	-0.29	0.44
	(-11.2)**	(-6.58)**	(-0.14)	(0.30)
Year 3	-2.17	-2.54	-2.68	-1.09
	(-2.96)**	(-1.49)	(-1.34)	(-0.74)
Year 4	-1.01	-2.46	2.27	-3.89
	(-1.37)	(-1.44)	(1.14)	(-2.65)**
Highest Wage				
Year 1	-.20	-.08	-.10	-.20
	(-1.30)	(-0.12)	(-0.22)	(-0.63)
Year 2	-.22	-.37	.33	-.25
	(-1.46)	(-0.82)	(0.59)	(-0.79)
Year 3	-.40	-.47	-.10	-.18
	(-2.69)**	(-1.04)	(-0.18)	(-0.56)
Year 4	-.49	-.09	-.55	-.09
	(-3.29)**	(-0.19)	(-0.97)	(-0.27)

** Statistically significant at the .01 level
* Statistically significant at the .10 level

During the participation or eligibility year, income is higher and unsubsidized employment is lower for eligibles, surely due to program participation--the average length of a program spell was about 15 weeks. Once again, wage rates are lower for eligibles.

In the years following participation, however, the income effects for participants become negative too, reflecting a smaller relative rise in unsubsidized employment but also a larger drop in subsidized employment and a large wage deficit. Both the income and the wage effects of participation are most negative in the second year following participation. The -.314 coefficient in the log income equation translates to 27 percent lower income for participants during the second year following.

Comparing these effects over the years male participants in the later years have income and wages not only lower than eligibles, but also relative to themselves in earlier years. While their unsubsidized employment rises relatively, this is almost surely due to reduced CETA participation partially but not fully replaced by other employment[7].

For females the results are better than for males but still not necessarily positive. Participation is related to higher earned income in all years, but not significantly so in the last year. Part of the advantage in earlier years may be due to greater labor force participation that is correlated with the CETA participation decision but not explained by other observable factors. Participation is also related to fewer weeks of unsubsidized employment, but this difference is not significant in the fourth year. From the income and employment standpoints, female participants gradually become more similar to the non-participants. However, female participants' wage rates are lower in all years, but significantly so only in the years following participation.

The pattern of impact coefficient changes is very similar to the year-to-year pattern in the male results, but all female coefficients are slightly more positive than the male ones. This pattern shows participants being somewhat more work-oriented, even in subsidized employment, than non-participants in the first two years of the spell, although the wage rates for those who work are lower. In subsequent years non-participants have relatively more labor market success. Although one is tempted to attribute this success to more time in school, weeks spent in school are directly controlled for in the regressions and have the expected negative effect on concurrent labor market outcomes. This pattern of coefficients also does not support the selection bias argument, which would have predicted that participants would be somewhat worse off on average before participation, but improved relative to non-participants as the labor market problems leading to CETA participation wore off.

The model containing the subsidized employment and summer program variables are included for comparison purposes, but a major problem is that the subsidized employment variable is highly correlated with the participation variable, increasing the standard errors and reducing significance for both. The time patterns of the participation coefficients are very similar to those in Model 1, although not as significant. The subsidized employment coefficients suggest that these participants are somewhat better off than other participants before the focal year, and worse off afterwards, but the pattern is not unambiguous and statistical significance is lower. The same problems occur in interpreting the summer coefficients, where summer participants seem to be relatively worse off both before and after the focal year.

VIII. SELECTION BIAS MODELS

The selection bias argument holds that unobserved differences between participants and non-participants led them to make decisions which may affect future outcomes. The characteristics underlying these unobservable differences can only be speculated upon, but could generally be called a "proclivity to participate." Some individuals may be prone to take advantage of government programs. It is also possible that individuals are experiencing particularly bad fortune in the labor market relative to their expectations, and that CETA looks more attractive or is one of the few options available to them.

If it is indeed bad fortune that leads people to choose CETA and that is the unobservable difference to be represented by a selection bias correction, our consideration of that correction is affected. Bad fortune, by definition, is a random factor, and although its effects may linger, it may also be replaced by better fortune in succeeding years. If we observe changes in outcomes between the pre-participation period and the post-participation period, we are likely to notice some improvement due merely to the replacement of bad luck with at least average luck. This kind of change should not be mistaken for a CETA participation effect, hence the need for the selection bias correction. However, CETA may help reverse that bad fortune more quickly, providing at least a quick, if temporary, boost to the participant. This kind of CETA effect would be covered up within the standard selection bias correction, but can be observed using a slight variation on the standard selection model.

A more formal characterization of the preceding argument is presented in Appendix 2. In short, selection of CETA is associated with negative outcome errors in the prior period. The temporary CETA effect is to reduce the normal correlation of errors between periods. If the correlation coefficient is below one, as is expected, this effect will get smaller over time.

To study these effects, models of outcome changes are examined. This tactic eliminates individual fixed effects and allows us to focus on the selection bias problem. Since both participants and non-participants are observed, and we wish to allow for a constant CETA effect represented by a dummy variable in the regression, the problem becomes one of an endogenous dummy variable, and its treatment is described in Barnow, Cain and Goldberger (1980) and can be estimated efficiently. While a model allowing the selection correction to affect one decision group differently than another decision group has been discussed by Lee (1979), no statistical package currently available handles it exactly as necessary here. However, results that are not corrected can be estimated simply via OLS using selection bias corrections estimated from a probit model for participation. These models will be presented here, along with change models not corrected for selection, for comparision purposes.

Table 1.7 displays the results of the probit models used to explain CETA participation. All variables used are measured in the focal year, unless otherwise noted. A number of the coefficients are statistically significant, but previous participation in a government program is perhaps the strongest, while being of minority status also makes it more likely that a person will participate. Interestingly, some factors affect males differently than females, such as education, health limitations, children, weeks in school in the prior year, and one of the regional dummy variables. The health and children variations are not surprising, and regional differences can occur for many reasons. The prior schooling and education differences would be explained if males were more likely to enroll in summer programs or come to

Table 1.7 CETA Participation Probit Estimation Results

(t-statistics in parentheses)

Variable	Male	Female
Constant	-.597 (-2.54)**	-.747 (-3.31)**
Age	-.0056 (-0.38)	-.0262 (-1.87)*
Education	-.0405 (-2.84)**	.0207 (1.44)
Married, Spouse Present	-.122 (-1.75)*	-.209 (-4.34)**
AFQT	-.0032 (-3.75)**	-.0024 (-2.64)**
Black/Hispanic	.367 (9.32)**	.495 (12.6)**
Health Limitations	.191 (2.39)*	.0044 (0.07)
Previous Participation	.936 (13.1)**	.829 (11.5)**
Weeks in School, Prior Year	.0053 (3.81)**	.0008 (0.59)
Children	.110 (1.95)*	-.095 (-3.17)**
Region 1	.155 (2.77)**	-.122 (-2.20)*
Region 2	.189 (3.51)**	.046 (0.86)
Region 3	-.103 (-2.17)*	-.114 (-2.46)*
χ^2 (d.f.)	354.6 (12)**	325.4 (12)**

** Statistically significant at the .10 level
 * Statistically significant at the .01 level

CETA shortly after dropping out of school, while female participants are returning to the work force after having been out of school for a while. This analysis reveals that some observable factors are strongly related to participation, with previous participation perhaps representing knowledge of government program availability or willingness to make use of government programs, so that any random factors remaining may be related to individual labor market aberrations or just luck.

Tables 1.8a and 1.8b give training variable and selection correction (lambda) coefficient estimates for outcome change models from year one to year three and year one to year four, with the assumption discussed earlier that year one is the appropriate--or at least best available--control year for fixed effects. Other variables in these models include age, age squared, children, highest grade completed, married spouse present, health limitations in the third or fourth year, changes in marital status, health, education, weeks in school from year one to the third or fourth year, AFQT score, minority status, and regional dummies. Six models are shown, one for each of the possible selection correction methods, with and without the summer and subsidized employment variables.

Both male and female participants show a drop in earned income after CETA in the uncorrected model, and this drop is seen to be more concentrated among those in subsidized employment. This finding is just the opposite of what selection theory would have predicted, assuming CETA does not have a negative effect. However, it does agree with the time series-cross section model results. Because the comparison of income means in Table 1.5a shows that average absolute income growth is about the same for the two groups, these results indicate that CETA participants are not experiencing the percentage growth that should be associated with their human capital characteristics.

Table 1.8a Training Variable Coefficients in Selection Models: Males
(t-statistics in parentheses)

	Participation	Lambda	Part. x Lambda	Summer	Subsidized Employment
Year 1 to Year 3 Changes					
Log Earned Income					
1)	-.434 (-3.07)**	--	--	--	--
2)	-.194 (-0.58)	--	--	-0.070 (-0.25)	-0.260 (-0.73)
3)	-2.98 (-4.28)**	1.54 (4.43)**	--	--	--
4)	-2.91 (-3.98)**	1.54 (4.44)**	--	.067 (0.24)	-.103 (-0.30)
5)	-3.42 (-4.19)**	1.29 (3.12)**	-.108 (0.25)	--	--
6)	-3.20 (-3.36)**	1.30 (3.13)**	-.110 (0.24)	.036 (0.13)	-.287 (-0.80)
Weeks of Unsubsidized Employment					
1)	1.26 (1.35)	--	--	--	--
2)	5.10 (2.34)*	--	--	-3.61 (-1.95)*	-3.36 (-1.43)
3)	0.90 (0.32)	0.04 (0.03)	--	--	--
4)	4.42 (1.35)	-.467 (-0.28)	--	-3.43 (-2.44)*	-2.03 (-1.14)
5)	-12.13 (-2.26)	1.97 (0.73)	4.31 (1.49)	--	--
6)	-7.28 (-1.26)	2.00 (0.73)	3.73 (1.28)	-2.97 (-1.59)	-3.45 (-1.46)
Highest Wages					
1)	-.146 (-0.56)	--	--	--	--
2)	.127 (0.20)	--	--	-.502 (-0.93)	-.190 (-0.27)

Table 1.8a (continued)

	Participation	Lambda	Part. x Lambda	Summer	Subsidized Employment
3)	-.212 (-0.15)	.161 (0.20)	--	--	--
4)	.372 (0.22)	.059 (0.07)	--	-.588 (-0.97)	-.323 (-0.39)
5)	-2.64 (-1.64)	.533 (0.61)	.571 (0.64)	--	--
6)	-2.28 (-1.29)	.526 (0.60)	.502 (0.56)	-.409 (0.75)	-.128 (-0.19)
Year 1 to Year 4 Changes Log Earned Income					
1)	-.544 (-3.70)**	--	--	--	--
2)	.056 (0.16)	--	--	.421 (1.43)	-.844 (-2.26)*
3)	-3.06 (-4.28)**	1.54 (4.27)**	--	--	--
4)	-2.69 (-3.55)**	1.56 (4.34)**	--	.556 (1.95)*	-.655 (-1.82)*
5)	-3.67 (-4.36)**	1.43 (3.36)**	-.212 (0.46)	--	--
6)	-3.25 (-3.59)**	1.46 (3.42)**	-.147 (0.32)	.531 (1.79)*	-.883 (-2.37)*
Weeks of Unsubsidized Employment					
1)	1.76 (1.78)*	--	--	--	--
2)	3.81 (1.65)*	--	--	-2.91 (-1.48)	-1.47 (-0.59)
3)	1.34 (0.48)	-.206 (-0.13)	--	--	--
4)	4.71 (1.44)	-.691 (-0.42)	--	-3.40 (-2.42)**	-1.92 (-1.08)
5)	-6.34 (-1.12)	1.14 (0.40)	2.68 (0.87)	--	--

Table 1.8a (continued)

	Participation	Lambda	Part. x Lambda	Summer	Subsidized Employment
6)	-3.47	1.18	2.19	-2.53	-1.54
	(-0.57)	(0.14)	(0.71)	(-1.27)	(-0.61)
Highest Wage					
1)	0.141	--	--	--	--
	(0.53)				
2)	-.051	--	--	-.420	.331
	(-0.08)			(-0.78)	(0.47)
3)	1.10	-.556	--	--	--
	(0.78)	(-0.69)			
4)	1.23	-.638	--	-.971	.235
	(0.74)	(-0.78)		(-1.61)	(0.29)
5)	-.832	.421	.038	--	--
	(-0.53)	(0.50)	(0.04)		
6)	-.944	.410	-.068	-.395	.336
	(-0.55)	(0.48)	(0.08)	(-0.72)	(0.48)

Table 1.8b Training Variable Coefficients in Selection Models: Females

(t-statistics in parentheses)

	Participation	Lambda	Part. x Lambda	Summer	Subsidized Employment
Year 1 to Year 3 Changes					
Log Earned Income					
1)	-.321 (-1.94)*	--	--	--	--
2)	.218 (0.57)	--	--	.281 (0.86)	-.742 (-1.78)*
3)	-5.40 (-5.20)**	2.97 (6.06)**	--	--	--
4)	-5.10 (-5.11)**	3.01 (6.19)**	--	.484 (1.64)	-.617 (-1.79)*
5)	-5.23 (-6.85)**	3.33 (6.96)**	-1.64 (3.33)**	--	--
6)	-4.78 (-5.53)**	3.34 (6.98)**	-1.63 (-3.28)**	.414 (1.26)	-.737 (-1.78)*
Weeks of Unsubsidized Employment					
1)	3.18 (3.56)**	--	--	--	--
2)	5.69 (2.76)**	--	--	-.895 (-0.51)	-2.67 (-1.19)
3)	.613 (0.20)	.900 (0.50)	--	--	--
4)	3.02 (0.86)	.572 (0.32)	--	-1.47 (-1.10)	-1.68 (-1.00)
5)	1.21 (0.29)	-4.02 (-1.55)	6.07 (2.27)*	--	--
6)	3.72 (0.79)	-3.93 (-1.51)	5.77 (2.14)*	-.442 (-0.25)	-2.39 (-1.06)
Highest Wage					
1)	-.176 (-0.84)	--	--	--	--
2)	-.117 (-0.21)	--	--	.204 (0.48)	-.117 (-0.20)

Table 1.8b (continued)

	Participation	Lambda	Part. x Lambda	Summer	Subsidized Employment
3)	-1.17 (-1.21)	.646 (1.12)	--	--	--
4)	-1.54 (-1.29)	.681 (1.16)	--	.025 (0.06)	.334 (0.52)
5)	-1.12 (-1.21)	-.332 (-0.53)	.895 (1.44)	--	--
6)	-1.31 (-1.17)	-.322 (-0.52)	.945 (1.50)	.343 (0.79)	-.009 (-0.02)
Year 1 to Year 4 Changes Log Earned Income					
1)	-.543 (-3.17)	--	--	--	--
2)	.115 (0.29)	--	--	.681 (2.01)*	-1.02 (-2.38)(
3)	-6.20 (-5.59)**	3.32 (6.3)**	--	--	--
4)	-5.91 (-5.65)**	3.41 (6.53)**	--	.918 (3.08)**	-.851 (-2.49)*
5)	-6.17 (-7.85)**	3.56 (7.26)**	-1.55 (-3.06)**	--	--
6)	-5.73 (-6.44)	3.56 (7.26)	-1.48 (2.89)**	.847 (2.49)	-.974 (-2.28)
Weeks of Unsubsidized Employment					
1)	4.15 (4.37)**	--	--	--	--
2)	6.46 (2.95)**	--	--	-3.23 (-1.72)*	-1.63 (-0.68)
3)	1.52 (0.49)	.371 (0.21)	--	--	--
4)	3.48 (1.00)	.066 (0.04)	--	-1.58 (-1.18)	-1.15 (-0.68)

Table 1.8b (continued)

	Participation	Lambda	Part. x Lambda	Summer	Subsidized Employment
5)	0.47	-2.23	4.69	--	--
	(0.11)	(0.81)	(1.66)*		
6)	3.47	-2.10	4.03	-2.84	-1.41
	(0.70)	(-0.77)	(1.41)	(-1.50)	(-0.59)
Highest Wage					
1)	-.326	--	--	--	--
	(-1.47)				
2)	-.060	--	--	.052	-.311
	(-0.10)			(0.12)	(-0.49)
3)	-1.99	1.08	--	--	--
	(-1.92)*	(1.69)*			
4)	-1.76	1.09	--	.243	-.340
	(-1.39)	(1.69)		(0.52)	(-0.50)
5)	-2.05	.043	.827	--	--
	(-2.08)*	(0.06)	(1.26)		
6)	-1.97	.049	.848	.228	-.206
	(-1.65)*	(0.05)	(1.28)	(0.51)	(-0.32)

The difference between absolute and percentage income growth is likely due to moderate growth for those who had above average incomes in the first year, resulting in somewhat smaller percentage growth, combined with little growth in the low income categories, where moderate absolute growth would have resulted in larger percentage growth. This finding, if valid, does not bode well for CETA participation, for it suggests that those who need help the most are not getting it, or at least are not benefiting from it.

The lambda variable here can be thought to represent either "participation proclivity" or "potential to rebound from bad fortune," or whatever selection bias represents. The lambda correction tends to be quite significantly positive in all income equations, indicating that to some degree bad luck is replaced by better luck. This correction, however, makes the participation coefficient even more negative, by accounting for some of the luck improvement that CETA would take credit for without the correction. The participation coefficient should not be interpreted separately from the lambda coefficient, however, because the method of estimation forces lambda to have a higher average value for participants than for non-participants. The fact that income changes are sensitive to changes in the value of lambda within decision groups as well as in the average difference between decision groups, which can be accounted for by the participation dummy, shows that there is a clear sensitivity to selection bias.

The CETA temporary effect theory would suggest that the participation-lambda interaction should have a positive coefficient if CETA helps participants respond from bad luck more quickly. All of the signs on that coefficients are negative, however, and many are significant. This result reinforces the notion that the participants in CETA who are really down on their luck, or alternatively, had major unseen labor market difficulties, show little improvement after CETA.

Analysis of changes in weeks in unsubsidized employment presents a qualification to the above story. While the income variable included income from subsidized jobs, which could allow changes in subsidized employment to affect outcomes, the unsubsidized employment variable measures labor market self-sufficiency to some degree. We find that both male and female participants show improvements greater than that of eligibles, especially females. This improvement is smaller for those who were in summer or subsidized employment programs, but it becomes smaller and less significant when selection bias is accounted for. Evidently those below-average in unsubsidized employment prior to the focal year are just catching up, replacing subsidized employment with unsubsidized employment. Since CETA participants were more likely to have had a prior spell of participation, this effect is now accounted for in the selection bias term rather than the participation coefficient. The good news for CETA is that the participation-lambda interaction has a positive, sometimes significant, coefficient, indicating that CETA participants recover faster. However, their recovery may happen because, after two spells of participation (the prior year and the focal year) people are reluctant to come back or are not taken back. Whether this interaction effect is valid depends on the extent to which subsidized employment's only alternative was non-employment.[9]

Wages changes for male participants tended to be slightly smaller than for eligibles, but in no model did any coefficient exhibit real significance. Summer participation had a consistently negative but small effect, while subsidized employment's effect depended on the years involved. The selection bias corrections also have changing signs and are difficult to interpret. This problem may be due to the added selection problem of labor market participation to provide an observed wage, although with males this should be less of a problem than with females. Females also displayed a wage decline, and coefficient estimates are sometimes significant in the year four change. Summer participation is now a consistently positive, but still small, factor, while subsidized employment generally has a small negative effect. The selection terms show that a positive value for the selection factor results in greater improvement in wages and that most of that effect lies with the participant group. Although the effect is weak, CETA participants are reversing their bad luck or other difficulties faster than non-participants. It is crucial to remember here that this effect only holds for those with observed wages--those who are not employed in one period or the other are not included in this particular equation. Thus, the true problem cases may be not represented here.

IX. SUMMARY AND CONCLUSIONS

This chapter has examined welfare, education, and labor market outcomes for CETA participants, CETA-eligible non-participants, and non-CETA eligibles for one year prior and two years after the year of CETA participation or eligibility. This CETA exposure occurred between 1979 and 1981. Each analysis studies both outcome levels and outcome changes, and focuses on differences in outcomes among the groups and how they might be explained. We pay close attention to reasons for heterogeneity or selection bias that could affect conclusions drawn about the effects of CETA.

CETA participants are more likely than others to receive welfare both before and after their participation spell. They also have a greater likelihood of staying on welfare if they are already receiving it at the time of participation, and a greater likelihood of moving onto welfare if they are not already receiving it. This result holds up despite controls for welfare dependency prior to participation, various human capital and demographic characteristics, and OMB-defined poverty status in the year under examination. Although one can never be sure that there is still no remaining initial heterogeneity between CETA participants and others that causes this result, the controls used are very strong. The possibility that CETA contributed to welfare dependency, perhaps by formally or informally improving information about sources of support, should be considered.

CETA participants do not exhibit any marked differences in educational outcomes compared to eligibles. Male participants receive marginally less overall schooling and are somewhat less likely to receive more schooling during the single spell, but their disadvantage is not significant. Female participants fare slightly better than their eligible counterparts, especially in terms of completing or returning to high school during the sample spell. Once again, however, their advantage over eligibles is not significant. Any sample heterogeneity is not manifested in educational outcomes, nor does CETA appear to have had a strong effect.

CETA participants had rather different patterns of income, unsubsidized employment and wage rates during the sample spell than eligibles had. Both a model designed to account for individual heterogeneity across the years of the sample spell while examining outcome levels and one designed to control for fixed individual effects and selection bias while estimating outcome changes yielded similar results. Other observable characteristics such as human capital and demographic variables and time in school were controlled in each model. CETA participants' income declines relative to that of eligibles; having started out somewhat higher than eligibles', other things equal, participants income falls after the CETA spell until it is lower. Weeks of unsubsidized employment, a slightly different measure than income in that it concentrates on labor market self-sufficiency, are lower for participants than for eligibles across all sample years. However, the disadvantage gets slightly smaller in the years after participation. Wage rates are also slightly lower for participants, and drop somewhat relative to eligibles following participation.

The relationship between the CETA effect and selection bias is examined in these labor market outcomes. Given that some people selected (or are selected into) CETA because they have been experiencing labor market fortune or bad fortune, some natural improvement is to be expected if such bad fortune fades over time and is replaced by at least average fortune. This natural improvement should not be mistaken as a CETA effect. However, CETA may play a role in reversing these difficulties more quickly than normal. By estimating a factor representing this "natural improvement potential" for both eligibles and participants, the difference in its effect between the groups is studied. This factor has the greatest effect in the income equations, where it explains a significant degree of the improvement observed. CETA participants seem to rebound less quickly from these difficulties, however. Some of our evidence suggests that CETA may help those experiencing little or no prior difficulty, but is not able to change significantly the outcomes for many of the true problem cases.

Selection bias considerations for weeks of unsubsidized employment show that CETA participants replace subsidized employment with unsubsidized employment faster than eligibles, partly because they had more subsidized employment in the prior year than eligibles. That they fully replaced their subsidized employment, or that they replace it with full-time jobs, is not indicated. While selection bias played no important role in explaining males' wage changes, female participants with initially low wages seemed to have this problem reversed more quickly by CETA participation. However, persons who did not work in either one of the years involved in the outcome change could not be included in the wage analysis, so the results apply only to those who had jobs in both years.

To the extent that this study has successfully dealt with heterogeneity and selection bias problems, it finds that CETA was not particularly successful in improving labor market outcomes and reducing welfare dependency for these youth. Further research might concentrate on examining specific subgroups of the youth population, such as those who had never worked prior to CETA, or labor market outcomes for those who came to CETA via welfare.

Chapter 1

Notes

[1] Examples are work on the Youth Incentive Enrollment Pilot Projects by Farkas, Smith, and Stromsdorfer (1983) and Jobs for Youth and 70001 Ltd. by Silkman, Kelley, and Wolf (1983).

[2] This poor prior period performance has been noted in other studies, e.g, Cooley, McGuire and Prescot (1979) or Silkman, Kelley and Wolf (1983). It is not necessarily the case here, though, depending on how one looks at the data. This phenomenon will be explored later in the chapter.

[3] A more exact representation of this effect of CETA on temporary problems and its relation to selection bias has been presented in Willke (1984).

[4] This limitation probably leads to effectively more strict eligibility criteria than CETA uses, so any bias in the sample should be in the direction of selecting income eligibles who are worse off than necessary.

[5] Some respondents participated in CETA in more than one of these years. The year with the most weeks involved in a CETA program was chosen in these cases. For those eligible in more than one year, the year in which the eligible was most similar to participants was chosen. This choice was accomplished by regressing, for each year in which one of the participants was eligible, a dummy variable indicating whether that year was the participation year or not on various individual characteristics and year dummies. These coefficients were used to predict for which year was the most appropriate control year for non-participant eligibles.

[6] Some observations had to be dropped due to incomplete or missing information on an important variable. Missing data are not a relatively serious problem, however.

[7] The CETA participation coefficient remains positive even when the amount of welfare received is controlled for instead of simple on-off welfare status.

[8] Hahn and Lerman (1983) get results similar to this, and reach the conclusion that CETA participants have a difficult time making the transition from subsidized to unsubsidized employment.

[9] Use of unsubsidized employment for analysis here is subject to an automatic bias resulting from the nature of participation and the choice of participation years. The biggest participation year in the NLS sample was 1978, but those spells are only in "prior years" for this study's purposes. Together with a tendency to have multiple spells, this fact results in participants being more likely to have subsidized employment in the prior year than in the later years of the spell. To the extent that subsidized employment is a substitute for unsubsidized employment, the latter type will obviously show an increase under these conditions. The extent to which this

is a valid CETA effect depends on whether subsidized employment's only alternative is non-employment. Analysis of total employment has just the opposite difficulty, because since it includes subsidized employment it may not indicate labor market self-sufficiency, and the income variable is tainted by that problem. Estimation of the same models as in Table 1.8 using changes in total employment yield results more similar to the income results, with one exception. The participation-lambda interaction coefficient is more likely to be significantly positive, suggesting that at least below average luck with respect to weekly employment is reversed more quickly for CETA participants. However, since income does not improve, hours worked or wages must not improve.

Chapter 1

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Appendix 1A.1

Some sample means for the edited sample are:

	Males		Females	
	Participants	Eligibles	Participants	Eligibles
Age*	19.1	19.7	19.2	19.6
Years of Schooling*	10.9	11.3	11.5	11.5
Health Limitations* (%)	7.0	5.1	7.9	8.0
Minority Status (%)	60.7	40.4	65.3	41.1
AFQT Score	55.2	63.9	57.9	64.2
Married, Spouse Present* (%)	7.9	13.2	11.4	21.4
Children*	0.09	0.11	0.28	0.39
Weeks in School*	25.9	22.6	26.7	22.3
Previous Participation (%)	23.0	5.1	19.9	5.0
1979 Focal Year (%)	36.6	48.4	42.1	51.2
1980 Focal Year (%)	35.8	32.3	33.1	31.7
Region 1 (Northeast) (*)	21.4	17.9	15.7	17.6
Region 2 (North Central) (%)	23.8	21.3	22.4	21.2
Region 3 (South) (%)	31.7	37.0	37.9	39.0
N	777	3217	750	3449

* Measured in focal year

Appendix 1A.1 (continued)

Sample means for the unedited sample are:

	Participants		Eligible		Non-Eligibles	
	Male	Female	Male	Female	Male	Female
Age*	19.5	19.6	20.4	20.5	20.7	20.7
Years of Schooling*	11.1	11.6	11.4	11.7	12.0	12.2
Health Limitations* (%)	5.9	9.0	5.1	10.2	2.9	6.1
Minority Status (%)	61.5	66.7	44.3	45.3	31.5	30.0
AFQT Score	55.9	58.0	62.3	62.5	71.7	72.3
Married, Spouse Present* (%)	9.2	12.7	15.9	25.5	18.6	33.1
Children*	.09	.32	.15	.55	.13	.32
Weeks in School*	22.5	23.1	17.2	16.2	17.5	16.8
Previous Participation (%)	77.5	80.9	14.6	14.2	6.2	7.1
1979 Focal Year (%)	38.8	43.1	32.0	32.2	35.0	34.0
1980 Focal Year (%)	35.2	35.5	35.7	35.5	31.7	31.4
Region 1 (Northeast) (%)	21.9	17.0	18.1	17.2	18.8	19.0
Region 2 (North Central) (%)	24.5	20.2	22.6	22.3	21.9	22.4
Region 3 (South) (%)	30.5	38.6	35.9	39.3	35.7	36.2
N	951	947	5509	5955	6959	6698

* Measured in focal year

Appendix 1A.2

Consider the following model. Let a labor market outcome Y_{it} for individual i in period t be represented by

$$(1) \quad Y_{it} = bX_{it} + b_T T_i X_{it} + c_i + e_{it}$$

where b and b_T are row vectors of coefficients, X_{it} is a column vector of observable individual characteristics in period t, T_i is a dummy variable for having recently participated in CETA, c_i is an unobservable individual fixed effect, and e_{it} is an unobserved random error that has the autocorrelation structure:

$$(2) \quad e_{it} = re_{it-1} + u_{it}.$$

To allow for a change in the autocorrelation structure due to training, set $r = r_p$ for recent participants and $r = r_n$ for non-participants with the hypothesis that $r_p < r_n$, i.e., the trainee gets a "fresh start."

If (1) is estimated as it stands, any differences in means between the participant and non-participant groups for the factors c_i and e_{it} would affect the estimate of b_T and be misinterpreted as a training effect. However, c_i can be estimated by estimating outcome changes:

$$\begin{aligned}(3) \quad Y_{it} - Y_{is} &= b(X_{it} - X_{is}) + b_T T_i X_{it} + e_{it} - e_{is} \\ &= b(X_{it} - X_{is}) + b_T T_i X_{it} + (r_n - 1)e_{is} \\ &\quad + (r_p - r_n)T_i e_{is} + u_{it}\end{aligned}$$

where s represents the period before training. Further, by assuming that the participation decision is related to expected future outcomes, some information on e_{is} can be inferred. A person will choose the option that yields the greatest expected utility. Utility is a function of expected income and information about each option. Let the increase in the perceived value of training due to information be $E_i = mP_i +1\ v_i$, where P_i is previous participation and v_i is a normally distributed unobserved factor. Hence, a person will participate if

$$E(Y_{it}|\text{training}) + E_i > E(Y_{it}|\text{without training}), \text{ or}$$
$$bX_{it} + b_TX_{it} + d_i + r_pe_{is} + mP_i + v_i > bX_{it} + d_i + r_ne_{is}, \text{ or}$$
$$b_TX_{it} + mP_i > (r_n - r_p)\ e_{is} - v_i$$
$$b_TX_{it} + mP_i > Z_{it}$$

where $Z_{it} = (r_n - r_p)e_{is} - v_i$ and is also normally distributed. Under the assumptions given, the participation can be estimated using a probit technique, and $E(Z_{it}|T_i)$ can be calcalated using the inverse of Mills' ratio. Finally, let $\lambda_i = E(Z_{it}|T_i)$, so $E(e_{is}|T_i) = \sigma_{ez}\lambda_i$, where σ_{ez} is the covariance between e_{is} and Z_{it}, which is nonzero if $r_p \neq r_n$. Hence, we can estimate

$$(4)\quad Y_{it} - Y_{is} = b(X_{it} - X_{is}) + T_ib_TX_{it} + (r_n - 1)\lambda_i\sigma_{ez} + T_i(r_p - r_n)\lambda_i\sigma_{ez} + U_{it}$$

This yields estimates of b, b_T, $(r_n - 1)\sigma_{ez}$ and $(r_p - r_n)\sigma_{ez}$. A test of significance of b_T and $(r_p - r_n)\sigma_{ez}$ can be seen as a test of the presence of direct and indirect training effects. However, r_p and r_n are not identifiable.

Although it is not done in this chapter, year t+1 can be used to identify all parameters. Consider:

$$
\begin{aligned}
(5)\quad Y_{it+1} - Y_{is} &= b(X_{it+1} - X_{is}) + b_T T_i X_{it+1} + e_{t+1} - e_{is} \\
&= b(X_{it+1} - X_{is}) + b_T T_i X_{it+1} + r^* e_{it} - r^* e_{is} \\
&\quad + (r^* - 1)e_{is} + U_{it+1} \\
&= b(X_{it+1} - X_{is}) + b_T T_i X_{it+1} + r^*(e_{it} - e_{is}) \\
&\quad + (r^* - 1)e_{is} + U_{it+1}
\end{aligned}
$$

Here we use $e_{it+1} = r^* e_{it} + U_{it+1}$, with r^* not necessarily the same as r_n because the length of time between periods s and t is probably longer, due to the intervening training, than the length of time between periods t and t+1. In addition, from (3),

$$e_{it} - e_{is} = Y_{it} - Y_{is} - b(X_{it} - X_{is}) - T_i b_T X_{it},$$

and $e_{it} - e_{is}$ can be estimated using values of b and b_T estimated in (4). Also, we present e_{is} using $\lambda_i \sigma_{ez}$ as before. Then (5) becomes:

$$
\begin{aligned}
(6)\quad Y_{it+1} - Y_{is} = {} & b(X_{it+1} - X_{is}) + b_T T_i X_{it+1} + r^*(\widehat{e_{it} - e_{is}}) \\
& + (r^* - 1)\sigma_{ez}\lambda_i + U_{it+1}
\end{aligned}
$$

This yields a direct estimate of r^*, which then identifies σ_{ez} from the estimate of $(r^*-1)\sigma_{ez}$, and then identifies r_p and r_n from the estimates of $(r_p-1)\sigma_{ez}$ and $(r_n-r_t)\sigma_{ez}$ in (4).

Chapter 2

Work Experience and Future Employment Prospects

by

Lisa Lynch

Youth unemployment in the United States, as in most other developed countries, continues to be a challenging issue for policy makers. Even though the economy appears to be recovering from the recent recession, young people in the United States are still experiencing historically high unemployment rates. Recent figures from the Bureau of Labor Statistics found the overall unemployment rate in the U.S. in the fourth quarter of 1982 to be 10.7 percent. However, for young workers age 16-19, the unemployment rate was 24.3 percent, compared with 16.2 percent ten years ago. For black youth the numbers were even bleaker, with 49 percent of black youths unemployed in 1982 compared with 33 percent in 1972.

Some argue that policy makers should not be particularly concerned about youth unemployment because it is simply part of a productive and efficient job search process. In other words, time spent unemployed for young people is time spent accumulating valuable information about the labor market. However, periods of unemployment may have long term employment consequences for two major reasons. First, the loss of valuable work experience may make it more difficult for youths to find employment. Labor theories such as human capital imply that since substantial investment in human capital should occur in the early years of work, early unemployment is particularly costly. If there is no investment in human capital during periods of non-employment, the entire earnings profile of the worker will be depressed.

In addition, dual labor market theory suggests that early unemployment might lead to poor work habits, weak labor force attachment, and general alienation from society. The unemployment experience itself may alter unemployed youths if they become more discouraged about their chances of finding a job, and this discouragement spills over into their job search behavior. In addition, employers may also use employment records as a signal of potential productivity.

The youth labor market experience can be analyzed in a variety of ways. One approach is to try to explain changes in youth unemployment using time series data. Recent studies using time series data by Feldstein (1973), Wachter and Kim (1982) and Clark and Summers (1982) have attempted to show how aggregate demand, relative wages, and demographic changes have affected both cyclical and secular trends in youth unemployment. One of the advantages of time series analysis is that it is possible to assess the impact on aggregate youth unemployment of those explanatory variables that government policies might have some impact on. The major disadvantage of time series analysis is the problem of endogeneity of the explanatory variables included in most analyses.

Using survey data, both cross sectional and longitudinal, we can explore a variety of issues. For example, Meyer and Wise (1982), using the National Longitudinal Study of 1972 High School Seniors, show the importance of educational qualifications and part-time work experience during school in determining the probability of being unemployed during the first four years after high school graduation. Rees and Gray (1982), using the 1976 Survey of Income and Education, have shown the impact of family characteristics such as the occupation of the head of the household and unemployment amongst other family members on the probability of being unemployed for young people age 17-20 still living with their parents.

In addition to these factors, studies by Leighton and Mincer (1982), using data from the NLS young men's cohort 1967-69, and Clark and Summers (1982), using data from the 1976 Current Population Survey, have investigated the impact of job changes or high rates of turnover on the unemployment experience. Specifically, Leighton and Mincer (1982) find the number of job changes is significantly and positively related to the number of unemployment spells. On the other hand, Clark and Summers (1982) point out that over half of all job changes occur without any unemployment at all. Many of the studies have also analyzed white/non-white and sex differentials in the labor market experience of youths and have found significantly higher unemployment amongst non-whites and females.

Besides analyzing the characteristics of the stock of unemployed young people, several studies have attempted to determine the factors influencing the duration of unemployment. Studies by Ellwood (1982), using data from the NLS young men's cohort, and Corcoran (1982), using the NLS young women's cohort, have attempted to investigate the "scarring effects" of lack of work experience in the years immediately following school leaving. Both papers find some evidence that after controlling for individual differences, early unemployment results in worse future employment and earnings prospects.

With the exceptions of Stephenson (1976) and (1982), Heckman and Borjas (1980), Flinn and Heckman (1982a) and (1982b), Lynch (1985), and Wolpin (1984), few studies on youth unemployment have linked theoretical developments of search theory with data on either the duration of non-work or unemployment of young people. This chapter begins to address this issue, using data from the NLS youth cohort.

The studies by Stephenson, Heckman and Borjas, and Flinn and Heckman analyzed data from the early years of the NLS young men's and young women's cohorts; thus their examination of the youth labor market focused in a period of relative economic prosperity. Utilizing the NLS youth cohort data, we can examine how the recession of the early 1980's affected young workers.

The theoretical model used here is briefly described in the next section, followed in section two by a description of the data. In section three we present our results and in section four briefly summarize our major findings.

I. THE MODEL

The theoretical framework we used for the following analysis is a simple job search model such as that presented by Mortenson (1970) and Lippman and McCall (1976). This model assumes that when a worker becomes unemployed, the expected duration of his or her unemployment (or inversely, re-employment probability) will depend upon two probabilities--the probability of receiving a job offer and the probability of then accepting the job offer. The probability of receiving a job offer will be determined by personal characteristics such as education and training and local demand conditions which are typically proxied by the local unemployment rate. The probability that an unemployed individual will then actually accept a job offer will be determined by the wage associated with that offer. Once an individual receives a job offer, he or she has to trade off the returns from accepting that offer with the costs of rejecting the offer in order to possibly find a better paying job. Therefore, there will be some minimal acceptable wage which equates the marginal benefits of accepting a job offer with the marginal costs of rejecting that offer and continuing job search. This wage is referred to as the reservation wage. An individual will reject all offers below this wage. Factors which will determine this wage include the potential distribution of wages, the costs of search, any unemployment income, and the probability of receiving a job offer. The lower an individual's qualifications, the lower will be the potential wages available and consequently the reservation wage will be lower. Higher costs of search (such as travel expenses to job interviews) will then lower the reservation wage. Higher unemployment income will raise the reservation wage because the loss of earnings due to unemployment becomes smaller. Finally, a lower probability of receiving a job offer will reduce the reservation wage because an unemployed individual knows that if a job offer is rejected, it may be a long time before another offer is obtained.

Intuitively this model is very appealing; however, there are some limitations. In particular, it has been argued that jobs have non-pecuniary benefits attached to them that are not observed through the wage associated with a job. Unfortunately, we have not been able to take such benefits into account in our analysis. The model we are using, however, has the advantage of not assuming that individuals are continuously receiving job offers, so we are able to examine how demand conditions affect the youth labor market experience.

Although we are using a search model to justify the inclusion of particular variables in our estimations, we are not estimating a structural model but a reduced form search model. This choice means that we will observe the total effect of variables on the duration of unemployment rather than the separate effects on the reservation wage and the probability of receiving a job offer. Because this type of estimation does not give us any direct insight into the role of reservation wages, our estimation cannot be viewed as a direct test of search theory. Our estimation does have the advantage of not making any specific distributional assumptions required in structural analyses.

II. THE DATA

For our analysis we have chosen to examine the determinants of the duration of periods of non-work for youths who were not employed at the 1982 interview. We have restricted our sample to those youths who were not employed but not in school or in the military. In addition, we have required that the individual's date of last enrollment in school was before the date of their last job and that they did not return to school in the year following the 1982 interview. Using the 1983 interview, we were able to determine whether or not youths not working at the 1982 interview were able to obtain a job during the following year. For those who were successful in finding employment, we know the completed spell length of their period of non-employment. For those still not working by the 1983 interview, we only observed an "uncompleted" spell. Approximately 80 percent of the boys and 63 percent of the girls have completed their spell of non-work by the 1983 interview. We concentrated initially on transitions from non-work to employment because of the difficulty of identifying the exact sequence of weeks spent unemployed or out of the labor force. There is evidence, however, (Gonul, 1985) for youths and particularly for boys, that there is little difference between weeks spent "unemployed" and "out of the labor force." We have also studied a smaller subsample of those youths who spent the entire non-working period searching for work. This group has been labelled "continuously unemployed."

Table 2.1 presents some selected characteristics of the sample we have examined. This discussion is based on the unweighted sample of youths from the NLS cohort, and it must be remembered that this sample is not representative of the youth population in general because the cohort is over-

Table 2.1 Characteristics of Those Not Working at the 1982 Interview

Variable	Boys (n=743) Mean or % of Sample	Girls (n=848) Mean or % of Sample
Duration of non-employment at 1982 interview	20 weeks	26 weeks
Duration of non-employment for those not working at 1982 interview (completed and uncompleted spells)	42 weeks	55 weeks
Above item-completed spells only	32 weeks (n=588)	38 weeks (n=532)
Duration of "unemployment" for those unemployed at 1982 interview as of 1983 interview (completed spells only)	24 weeks (n=259)	18 weeks (n=133)
Age	20	21
School (years)	11	11.5
Local unemployment rate	11	10.5
Weekly pay (derived from earnings equation)	$ 181.27	$ 120.30
Weekly pay in last job	$ 165.67	$ 135.64
Weekly unemployment income (including those who receive no U.I.)	$ 2.70	$ 1.52
Nonwhite	36	31
Married	14	41
Ill health	6	14
Live at home	63	38
Training (all except government)	29	31
Training (government)	7	6
On temporary layoff	14	6
Live in SMSA	70	71

represented with minority and low-income households. The first item in Table 2.1 shows the average number of weeks of non-employment for youths not working as of the 1982 interview. The second item indicates the number of weeks of non-employment as of the 1983 interview for this group for both completed and uncompleted spells. For the boys this figure is 42 weeks, while for the girls the average spell length is 55 weeks. The differential between the girls and the boys is probably underestimated by these numbers since we know that only 63 percent of the girls have completed their spells by the 1983 interview. Items two and three show the danger of discarding data on uncompleted spells. By ignoring data on uncompleted spells, we would be underestimating spells of non-work by at least 25 to 30 percent for boys and girls respectively. The fourth items in Table 2.1 examines a subsample of 35 percent of the boys and 16 percent of the girls who searched continuously for a job after they began their spell of non-employment and completed their spell of unemployment by the 1983 interview. Continuous search reduces the averaged completed spell length for the boys by 25 percent and by a dramatic 53 percent for the girls. Other major diferences between the two groups are the lower pay for full time female workers and the much higher percentage of non-working girls who were married compared with the boys (41 percent of girls versus 14 percent of boys). It is not surprising then to see that only 38 percent of the girls but 63 percent of the boys were still living with their parents. Because of these differences in household characteristics, we have analyzed boys and girls separately.

It is sometimes argued that the majority of young people go in and out of work quite frequently and a small minority spend a longer period of time out of employment. However, as the means on the number of weeks spent not working in Table 2.1 and the frequency distribution presented in Table 2.2 indicates, this description is probably not accurate for the labor market experience of our sample. In Table 2.2, 18 percent of the boys and 14 percent of the girls have spells of non-work less than three months. Over a third of the boys and over 55 percent of the girls have spells of non-work greater than 52 weeks.

Our theoretical model implies that the re-employment probability (which we proxy by the log duration of a spell of non-work) is determined by the probability of receiving a job offer and the probability of accepting that job offer. By using logs, we can interpret the coefficients on our independent variables as the percentage change in the duration of unemployment due to a one percent change in the independent variable. Given the distribution of wages, the factors which will be the most likely to influence the probability of receiving a job offer are local demand conditions (proxied here by the local unemployment rate) and personal characteristics. We expect that non-whites might experience longer durations of non-work, especially if they are being discriminated against in the labor market. Other variables which might determine the probability of receiving a job offer include human capital variables such as whether or not the individual had any training (vocational, technical, other skills, or governmental training programs) and the completed number of years of schooling. If an individual is on temporary lay-off, we would also expect that their duration of non-work would be shorter. Other variables we have used include whether or not the individual lives in an SMSA (the "inner city" problem), whether or not they are healthy, whether or not they live at home (a proxy for search intensity or parental pressure to get a job), age, whether or not they are married, and the length of the last spell of non-work.

A variable which is assumed to have a direct effect upon the reservation wage but not on the probability of receiving a job offer is unemployment

Table 2.2 Frequency Distribution of the Number of Weeks of Completed and Uncompleted Spells of Non-Work by Interview 1983

Weeks	Boys	Girls
1-4	7.3	6.8
5-8	6.1	3.8
9-12	5.1	2.9
13-24	15.7	11.0
25-52	32.6	23.8
52 +	33.2	51.7

income. Unemployment income subsidizes job search so that the more unemployment income an individual receives, the higher the reservation wage, ceteris paribus. Our analysis uses the average amount of unemployment compensation the respondent received while unemployed. Many of the respondents do not receive unemployment compensation, primarily because they are not eligible for it, but also because they sometimes do not claim it even when eligible. In previous studies researchers have included unemployment compensation in a ratio of unemployment income to expected earnings. This "replacement ratio" has no specific justification within a search framework, so we test this ad hoc restriction in our estimation.

An individual's reservation wage will also be determined by the expected mean of the wage distribution facing him or her. To approximate the mean of this distribution we use earnings in last job or a predicted earnings variable obtained from an earnings equation estimated for all employed youths in the cohort at the 1983 interview. As Nickell (1979) has noted, it is more appropriate to use a fitted value from an earnings equation than any actual level of income, which will be endogenous (see the appendix for further details).

III. THE RESULTS

The following tables present our findings on the determinants of the duration of non-work for boys and girls. As discussed in the previous section, we tested whether or not the ad hoc restriction [illegible] the replacement ratio held. Because we found that for almost every equation the replacement ratio restriction was rejected, we only report findings with the two variables included separately. Tables 2.3 and 2.4 present OLS estimates of the determinants of the log duration of both completed and uncompleted spells of

Table 2.3 Determinants of the Log Duration of Uncompleted and Completed Spells of Non-Work: Boys

Variable	Equation 1 Estimate	Equation 1 T-test	Equation 2 Estimate	Equation 2 T-test
Constant	13.45	7.86*	4.06	6.31
Nonwhite	0.03	0.32	0.21	2.51*
Age	0.15	4.86*	0.02	0.77
School	0.04	1.51	-0.04	-1.45
Married	0.15	1.09	-0.20	-1.49
Log expected weekly earnings (1)	-2.67	-6.20*		
Log expected weekly earnings (2)			-0.17	-1.94*
Log weekly UI	0.05	2.42*	0.05	2.34*
Healthy	0.05	0.29	-0.13	-0.76
U Rate	0.01	1.00	0.04	3.30*
Live at home	-0.19	-2.17*	-0.18	-1.95*
Train (VTS)	0.17	1.83	-0.04	-0.43
Train (Gov)	0.17	1.12	0.07	0.46
Temp layoff	-0.80	-6.70*	-0.94	-7.74*
SMSA	0.34	3.47*	0.07	0.80
Length of last spell of non-work	-0.00	-1.53	0.00	0.67
Number of cases = 743				
R^2	0.14		0.11	

(1) Predicted values of log expected weekly earnings obtained by using earnings equation in appendix.

(2) Wage in last job used to proxy for log expected weekly earnings.

Table 2.4 Determinants of the Log Duration of Uncompleted and Completed Spells of Non-Work: Girls

Variable	Equation 1 Estimate	Equation 1 T-test	Equation 2 Estimate	Equation 2 T-test
Constant	12.87	7.82*	3.03	5.45*
Nonwhite	0.15	1.79	0.26	3.21*
Age	0.07	3.84*	0.07	3.53*
School	0.07	1.95*	-0.09	-3.79*
Married	0.28	3.22*	0.15	1.82
Log expected weekly earnings (1)	-2.47	-6.15*		
Log expected weekly earnings (2)			0.01	0.24
Log weekly UI	0.04	1.57	0.02	0.77
Healthy	0.17	1.46	-0.17	-1.68
U. rate	0.006	0.53	0.03	2.61*
Live at home	-0.12	-1.39	-0.12	-1.34
Train (VTS)	-0.08	-0.97	-0.20	-2.65*
Train (Gov)	-0.03	-0.24	0.005	0.04
Temp. layoff	-1.44	-9.49*	-1.54	-9.92*
SMSA	0.26	2.93*	0.04	0.43
Length of last spell of non-work	-0.0004	-1.65	0.002	0.98
Number of cases= 848				
R^2		0.20		0.16

non-work for boys and girls respectively. Equation one presents results using the earnings equation, shown in the appendix, to derive expected earnings. Alternatively, equation 2 uses the wage from the last job with all of its associated problems of endogeneity. Surprisingly, we obtain a better fit with the data from the earnings equation than with the wage in the last job.

Several interesting findings are presented in the tables. If we focus on the first equations presented in Tables 2.3 and 2.4., which use the earnings function to generate predicted expected earnings, statistically significant determinants of the duration of non-work for boys include age, expected earnings, unemployment income, living at home, being on temporary lay-off, and living in an SMSA. For the girls the results are slightly different, with marital status being statistically significant in the length of their non-work spell and unemployment income insignificant.

These results are very sensitive to the choice of variables used to proxy for expected earnings. If we use wage in last job rather than a predicted value from the earnings equation, the results as shown in equation 2 alter for both boys and girls. Variables such as the local unemployment rate and ethnicity become statistically significant. Since these two variables are also significant determinants of earnings, their impact on the log duration of unemployment in equation 1 is probably being picked up entirely through the predicted earnings variable.

The parameter estimates shown in Tables 2.3 and 2.4 are not particularly intuitive. Therefore, in Table 2.5 we show how the average duration for a spell of non-work for our sample is sensitive to changes in variables of interest. Although unemployment income is a statistically significant determinant of the length of non-work, especially for boys, the numbers in Table 2.5 show that the overall impact of unemployment income is very small.

Table 2.5 Sensitivity of Average Duration of Non-Work to Changes in Explanatory Variables

Change in Explanatory Variable	Average Duration of Non-Work (Completed and Uncompleted Spells)	
	Boys	Girls
Mean Values	42 weeks	55 weeks
10% increase in unemployment income	43 weeks	56 weeks
50% increase in unemployment income	46 weeks	59.5 weeks
1% increase in expected earnings	38 weeks	50 weeks

Even a 50 percent increase in unemployment income will only increase the average spell of non-work from 4 to 4 1/2 weeks for boys and girls respectively. On the other hand, very small increases in expected earnings can reduce spell lengths by 4 weeks for the boys and 5 weeks for the girls. Important determinants of expected earnings include schooling, training, the local unemployment rate, and previous work experience.

Tables 2.6 and 2.7 present OLS estimates of the determinants of the log duration of completed spells of non-work and continuous unemployment for boys and girls. Equations 1 and 2 again examine the two proxies for the expected wage, and equation 3 restricts the sample to those who stated that they were continuously searching for work during the entire non-work spell. Perhaps one of the most significant results is the increased importance of unemployment income for girls who searched continuously for employment as shown in Table 2.7, equation 3. However, even for this subsample, if unemployment income was increased by 10 percent their average duration of unemployment would only rise from 18 to 19 weeks.

IV. SUMMARY

In the above analysis we have tried to identify significant factors which affect the length of non-work periods for youths. We have grouped together time spent out of the labor force but not in school or military service and time spent unemployed, and redefined this as non-work time. Our results indicate that this grouping may be more appropriate for boys than for girls. Of those youths with completed spells of non-work, 44 percent of the boys searched continuously for employment while only 25 percent of the girls searched continuously. For the girls, being married increased the duration of non-work; yet being married decreased the duration of time spent continuously

Table 2.6 Determinants of the Log Duration of Completed Spells of Non-work and Unemployment: Boys

Variable	Equation 1		Estimate 2		Estimate 3	
	Estimate	T-test	Estimate	T-test	Estimate	T-test
Constant	13.97	7.35*	4.11	5.63*	14.34	4.43*
Nonwhite	-0.08	-0.76	0.14	1.42	-0.02	-0.09
Age	0.14	4.20*	0.00	0.05	0.16	2.72*
School	0.07	2.18*	-0.01	-0.42	0.07	1.13
Married	0.23	1.51	-0.12	-0.83	0.52	2.17*
Log expected weekly earnings (1)	-2.84	-6.16*			-3.19	-4.19*
Log expected weekly earnings (2)			-0.25	-2.58*		
Log weekly UI	0.07	2.68*	0.07	2.73*	0.15	4.15*
Healthy	0.25	1.17	0.08	0.37	0.86	2.00*
Local U rate	-0.00	-0.01	0.03	2.49*	-0.01	-0.35
Live at home	-0.13	-1.28	-0.11	-1.02	-0.14	-0.75
Train (VTS)	0.17	1.70	-0.03	-0.27	0.20	1.16
Train (Gov)	0.14	0.78	0.04	0.22	0.48	1.57
Temp. layoff	-0.90	-6.97*	-1.03	-7.90*	-0.59	-3.36*
SMSA	0.33	3.07*	0.06	0.63	0.28	1.58
Length of last spell of non-work	-0.01	-1.99*	0.00	0.27	-0.01	-1.75
Number of cases	588		588		259	
$\bar{R}^2$	0.16		0.12		0.18	

(1) Predicted expected earnings variable obtained using the earnings equation in presented in the appendix.

(2) Wage in last job used for expected earnings.

Table 2.7 Determinants of the Log Duration of Completed Spells of Non-Work and Unemployment: Girls

	Equation 1		Estimate 2		Estimate 3	
Variable	Estimate	T-test	Estimate	T-test	Estimate	T-test
Constant	13.49	6.22*	1.79	2.44*	15.60	2.86*
Nonwhite	0.03	0.31	0.18	1.67	-0.16	-0.55
Age	0.09	3.68*	0.08	3.06*	0.16	2.52*
School	0.11	2.45*	-0.07	-2.17*	0.04	0.34
Married	0.18	1.57	0.08	0.64	-0.59	-2.03
Log expected weekly earnings (1)	-2.88	-5.42*			-3.69	-2.85*
Log expected weekly earnings (2)			0.09	0.86		
Log weekly UI	0.07	1.88	0.03	0.89	0.21	3.11*
Healthy	0.30	1.77	-0.10	-0.65	0.73	1.46
Local U rate	0.003	0.23	0.03	1.93*	0.03	0.82
Live at home	-0.05	-0.47	-0.04	-0.31	-0.24	-0.85
Train (VTS)	-0.002	-0.03	-0.17	-1.66	0.11	0.46
Train (Gov)	-0.02	-0.07	0.00	0.01	0.35	0.80
Temp. layoff	-1.31	-7.57*	-1.43	-7.99*	-0.68	-2.59*
SMSA	0.32	2.68*	0.06	0.48	0.39	1.47
Length of last spell of non-work	-0.01	-2.75*	-0.00	-0.42	-0.03	-2.97*
Number of cases	532		532		133	
$\bar{R}^2$	0.18		0.13		0.23	

(1) Predicted expected earnings variable obtained using the earnings equation in presented in the appendix.

(2) Wage in last job used for expected earnings.

unemployed. This finding suggests that non-market activities of girls affects their job search behavior in very different ways than for boys.

The consistent importance of being on temporary layoff suggests that researchers need to take layoffs into account when explaining job search behavior. Clearly being on temporary layoff with a high probability of recall will reduce the duration of non-work even if the individual is not actively searching for employment. Our findings also show that although unemployment income is a statistically significant determinant of nonemployment duration, its impact on the average duration of non-work is very small, even with large increases in unemployment compensation.

Tables 2.1 and 2.2 indicate that many young people seem to be spending very long periods of time out of employment but not in other non-labor market activities such as schooling or military service. This idleness is cause for concern especially because work experience is an important determinant of earnings (see appendix I).

Chapter 2

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Appendix 2A.1 Characteristics of Those Employed--Interview 1983

Variable	Boys (n=3596) Mean or % of sample	Girls (n=3208) Mean or % of sample
Weekly wage	$ 206.44	$ 174.16
Age	22	22
School (years)	12	12.5
Local unemployment rate	9.9	9.9
Work experience since since 1978 (weeks)	161	155
Nonwhite (%)	25	26
Training (%)	35	38
Married (%)	21	29
Ill Health (%)	3	6
Live in SMSA (%)	73	74

Appendix 2A.2 Determinants of Log Weekly Earnings--Interview 1983

Variable	Boys Estimate	Boys T-test	Girls Estimate	Girls T-test
Constant	5.21	7.23*	4.55	6.04*
Age	-0.09	-1.36	-0.05	-0.65
Age squared	0.003	1.83	0.001	0.82
School	0.03	7.34*	0.05	9.15*
Training	0.09	5.46*	0.05	3.24*
Nonwhite	-0.06	-3.44*	-0.02	-1.24
Married	0.11	5.16*	0.03	1.68
Healthy	0.06	1.30	0.13	3.82*
Local unemployment rate	-0.009	-4.09*	-0.009	-3.60*
SMSA	0.10	5.75*	0.08	4.25*
Work experience since 1978	0.002	14.56*	0.002	14.90*
$\bar{R}^2$	0.21		0.18	

Chapter 3

The Value of Time and Wage Offers: Selection in Longitudinal Data

by

Randall Olsen

I. INTRODUCTION

Selection bias is an important topic for applied researchers because it can distort the meaning of data which might otherwise seem straightforward. Although selection bias affects a number of issues in labor economics, this study focuses on its effect in interpreting wage rate data.

Wage rates inevitably influence decisions whether to work and how many hours to work. In addition, seeing how rates of pay influence opportunities in the labor market has relevance for policy makers. For example, to what extent are members of a particular group (women, minorities) disadvantaged in the sense of not being able to command as high a wage rate as persons whose other measured characteristics are identical? What is the role of changes in rates of pay upon hours worked in the labor force? Marginal changes in tax rates in effect change the wages workers earn for the last hour of work, and given time to adjust, such a change may affect the number of hours the worker chooses to work. "Static" analyses of the effects of tax changes ignore such adjustments, whereas a more dynamic analysis can recognize and attempt to quantify the dynamic response on labor supplied in the market and hence the effect on total output.

The basic selection bias problem arises because while some people are unable to find work, others do not work because they do not choose to work: the wage they can earn is quite low and they find it more advantageous to work in the home or to pursue other activities outside the formal labor market. When we examine the wages for either women and blacks or youths, many of whom are not employed in market jobs, such a pattern must be borne in mind.

Consider the problem of male/female wage differentials. If for persons age 25-30 with a high school education we observe the average wage of working men is \$5.50/hour and for women it is \$4.90/hour, does this mean that on average for persons 25-30 with a high school education men earn \$0.60 more than women? Not necessarily, for if all (or very nearly all) of the men in this category work, but only 55 percent of the women, we must consider why 45 percent of the women are not working. If the reason they do not work is that they value their time outside the home sufficiently highly that it is not worth their while to work at the wage they can earn in the market, it may be that the average wage which the non-working women could earn is below \$4.90/hour. If this is the case, the earnings differential between men and women could be in excess of \$0.60/hour. The fact that the wages of working women may not be representative of the wages of all women creates selection bias. (It should also be pointed out that it is possible in the example above to have a result that the true earnings differential is less than \$0.60/hour.)

The complications of selection bias imply that simple sample means on wage rates may be very deceptive. Because average earnings for one group or another are often used in policy debates, selection bias can skew such debates if average earnings are estimated substantially higher or lower than they really are. Concern over selection bias has led many researchers to adopt sophisticated corrective econometric techniques, which often produce very different results from those produced by simpler methods. These "corrected" results can yield very different conclusions, thus confusing issues in debates on social policy. In addition to the debate surrounding male/female wage differences, for example, the relative effectiveness of job training programs is another policy issue about which differing empirical conclusions can in part be attributed to differences in the treatment for selection bias.

The study of wage offers and labor force participation in this chapter differs in several respects from approaches to this problem which have appeared in the literature previously, but even with these differences the overall approach is similar to traditional labor/leisure research. It is assumed that the individual has a wage offer at every moment so that, if he or she wished, that person could work in the market place ot the offered wage. The decision whether to work is guided by a comparison of the wage offered in the formal market place and the value which the person attaches to time devoted to work at home or leisure. This paradigm is central in the static labor supply literature. Studies based upon cross-sectional data typically specify a wage offer equation and the reservation wage generates selection bias.

Methods for dealing with selection bias in cross-sectional data seek to determine whether wage rates are unusually high (or low) when a person is working and if so, this is the measure of selection bias. If wage offers are not systematically higher (or lower) when a person is working, then selection bias is not present and the wage offer equation can be estimated by ordinary least squares. The method used here seeks to detect selection bias by determining whether periods of strong wage growth are a associated with longer spells of employment than periods of low wage growth (or even a decline in the wage). If periods of rapid wage growth are associated with long employment spells, that association suggests that working persons will have unusually high wage offers, and hence selection bias is present.

This chapter shows how longitudinal data from the NLS can be used to better understand whether wages of young people who are working are roughly representative of the wages of young people who are not working. Changes in wage rates are related to movements into and out of jobs, and we find that for the sample under study there does not appear to be any selection bias. Traditional methods of studying selection bias using non-longitudinal data generate estimates of the average wage which a young person can earn in the labor market which is 60 percent higher than the average wage which is observed for youths who are actually working. The method of estimation in this study, while fairly sophisticated, yields an estimate of the average wage for all youths which is essentially equal to the average wage earned by those working. A difference of 60 percent in earnings opportunities in the labor market is clearly important to policy makers, and because methods of estimation which account for selection bias can affect inferences by such large amounts, this methodological issue bears directly upon policy debates.

The next two sections are a formal presentation of this study. Readers unfamiliar with or uninterested in statistical methods may wish to skip to section IV.

II. TECHNICAL BACKGROUND

The interrelation between the decision to work and wages has been the focus of a great many studies. A number of them concern the econometric problems which arise because wages are not observed for nonworkers.

Methods of estimation which take selection bias into account often produce unusual results which are very different from results derived with least squares.[1] Most work on selection bias estimates a binary outcome model which determines whether another variable is observed. The random component for the regression equation of this other possibly unobserved variable may be correlated with the random component determining whether this variable is to be observed. It is this correlation which produces selection bias. When selection bias is present, ordinary least squares is inconsistent.

When these sample selection methods are applied to wage offer models, it is commonly objected that they only consider the outcomes for labor force status and observed wage at a single time, whereas over a longer span of years a rather small fraction of the population never works and so does not have any wage information available. According to this objection, if one were to replace missing wages with future wages, the scope for selection corrections to change the estimated wage model would be reduced. The qualitative nature of the objection is valid, but one must still take into account that when a person chooses to work over the life cycle will be influenced by wage opportunities.

Heckman and MaCurdy (1980) estimate a dynamic model of labor supply over time which jointly estimates wage offer and Tobit labor supply equations for eight years of data. They estimated equations for labor supply during the year and the wage rate for those persons who worked. Their discrete time model used fixed effects in both the labor supply and wage offer equations, and found evidence of selection bias.

Here we use a continuous time construction which is modeled using Weiner processes. We attempt to determine whether random components of the wage offer process are correlated with the random components of the working-not working decision by examining whether fluctuations in the wage are systematically related to changes in working status. We specify a continuous time version of the usual labor/leisure/wage rate model which is closely related to the conventional cross-section models estimated using data on individual behavior at the time of a single interview. In fact, our initial observation on labor force status and the wage offer (if it is observed) contributes a term to the likelihood function which is identical to the likelihood for conventional cross-section wage offer-participation models. In these conventional models, selection is taken into account by assuming the wage offer regression error and the error term in the probit participation model have a bivariate normal distribution.[2]

It is necessary here to assume that the nonrandom component of the reservation wage is constant through time in order to obtain tractable distributions for the duration of spells of employment and nonemployment. The same statistical difficulties force us to use the available wage data suboptimally.

After the first observation on employment status and the wage (if observed) subsequent transitions into and out of employment are generated by assuming the random component in the probit-like employment/nonemployment model follows a Weiner process. This assumption implies that the distribution of spell lengths is governed by a Weiner first passage time distribution.

III. THE STOCHASTIC SPECIFICATION

As is often the case in the labor force participation literature, there is no discussion of the intensity of work effort (hours worked per week) here. Since the data are weekly, a week is considered, for practical purposes, as an instant. As is the practice in this literature, when we speak of wage offers and reservation wages, one should assume natural logs have been taken.

A. Extension of the Static Labor-Leisure Model to Continuous Time

Cross-sectional models of labor force participation often start with a reservation wage equation which gives the value of a person's marginal hour of time devoted to leisure (or production of some unobserved good or service in the home) when the person is not working in the market. The person works if the wage which can be earned in the market exceeds this reservation wage. Variation in labor-force participation accounts for a great deal of the variation in labor supply across persons. Even some of the observed variability in hours worked per year among those persons who work during a year is generated by variation in the fraction of time a job is held. This fact, together with the importance attached to the issue of unemployment, makes the explanation of the fraction of time a person holds a job arguably the most important issue in applied labor economics. We will model both the reservation wage and wage offer equations as Weiner diffusion processes. We will interpret the wage offer equation as showing the highest wage a person can receive in the market place at that instant.[3] Furthermore, we assume the person may enter or leave employment any time it is to the person's advantage to do so and whether one is employed does not affect the last wage offer. A more common approach is to view wages as arriving at irregular intervals with the worker having to either accept or reject the wage offer.[4] Within these more usual models the estimated rate at which wages arrive probably conceals the fact that many workers could obtain job offers on very short notice even though those jobs may pay poorly or be so disagreeable that the worker would not accept them. Whether wage offers arrive at irregular intervals and then expire if not immediately accepted, or there is always a best outstanding wage offer, is probably a difference more in style than substance. It will likely require a highly structural model with strong assumptions to reject one view in favor of the other, because the difference in the two views rests upon the continuous existence of wage offers which are not acceptable. The assumptions made here remove the complications of dynamic optimization. It is hoped that future work will integrate dynamic optimization into the stochastic framework here.

The highest wage offer is assumed to follow a Weiner-Einstein process. Let Y(t) be the best wage offer at time t. The increments to the Y process are assumed to be independent and a function only of the length of the increment of time and not the time at which the increment begins, i.e., the distribution of increments is stationary. $Y(t)-Y(s)$ is assumed to be normally distributed with mean zero[5] and variance $\sigma_y^2|t-s|$. If $Y(0) = \mu$; then the distribution of Y(t) is

$$f(y,t) = \frac{1}{\sigma_y\sqrt{2\Pi t}} \exp\{\frac{-(y(t) - \mu)^2}{2\sigma_y^2 t}\}$$

and the covariance between Y(t) and Y(s) is $\sigma_y^2[\mathrm{Min}(t,s)]$. σ_y^2 is referred to as the infinitesimal variance of the process. The longer this process has been in operation the greater the variance in the level of the process. Equivalently, only small changes in the maximum wage offer occur in small intervals of time.[6] There may be a great many potential employers of the worker, and new information from the other input and output markets may lead these potential employers to revise their assessment of the value of the marginal product of the worker. The fact that only small changes in the best wage offer occur in small intervals of time, would seem to imply that only small changes in other input and output prices occur in small intervals of time.

Just as in the static model, we postulate an equation representing the value of the person's time V(t) at zero hours of employment at time t with the decision whether to work being based upon a comparison of the value of time and the wage offer. Instead of the usual razor's edge static model in which the person works or not according to the sign of $Y(t)-V(t)$, we assume that the working person ceases to work if $Y(t)-V(t) < 0$, and the nonemployed person works when $Y(t)-V(t) > k$, where k is an unknown parameter to be estimated. k must be introduced to have an estimable model, as will be discussed below. The value of k may be thought of as the amortization of certain fixed costs associated with starting an employment spell. There is good reason to believe the size of k depends upon the prospective length of an employment spell, but we will simplify and assume a constant value for k through time. This assumption would be correct if there were no heterogeneity in the diffusion process, either across persons or for the same person through time. Since our model does not possess such heterogeneity it is consistent with a constant k. $V(t)$ is also assumed to be a Weiner-Einstein process with independent and stationary increments. $V(t)$ is assumed to have an unconditional mean μ_2. If we adopt the notation

$$Y(t) = \mu_1 + e(t)$$
$$V(t) = \mu_2 + u(t)$$

the working person ceases to work as soon as

$$\mu_1 - \mu_2 < u(t) - e(t)$$

and the nonworker starts a spell of employment when

$$\mu_1 - \mu_2 - k > u(t) - e(t).$$

Our assumption that the unconditional mean of the value of time does not change over time is admittedly strong. If the value of time has a linear time trend the method of estimation here would not allow identification of the behavioral parameters such as k and σ_u^2. One could still identify the wage offer process's parameters.

Because the diffusion processes for Y(t) and V(t) have the characteristic that only small changes are observed in small intervals, as soon as a person stops working we know $u(t) - e(t) = \mu_1 - \mu_2$ and when a person starts working $u(t) - e(t) = \mu_1 - \mu_2 - k$. The duration of a spell of nonemployment is the time it takes the composite Weiner-Einstein error process $x(t) = u(t) - e(t)$ to first reach $\mu_1 - \mu_2 - k$ starting from $\mu_1 - \mu_2$. The distribution of first passage times from z_1 to z_2 is only well defined for normal diffusion processes when $z_1 \neq z_2$, which explains why we need $k \neq 0$.

B Distribution of First Passage Times

Let $f(x_0,s;x,t)$ be the probability density function for the location of a diffusion process, x, at time t given the process starts at x_0 at time s. We wish to find the distribution of the duration of time for a diffusion process to first reach the value $x_0 + d$, call it $g(x_0,s;x_0 + d,t)$. Now any path from x_0 to x must transit through x,[7] so we have the fundamental identity

$$f(x_0,s;x,t) = \int_0^t g(x_0,s;x,\tau)\, f(x,\tau;x,t)\,d\tau$$

which enumerates all the paths from x_0 to x by integrating over all the possible first passage times τ from x_0 to x times the density associated with the process starting at x at time τ and returning to x at time t. Exploiting the stationarity of the diffusion process, we can express this identity as

$$f(0,0;x,t) = \int_0^t g(0,0;x,\tau)\ f(0,0;0,t-\tau)d\tau$$

The ab initio derivation of the form of the distribution of first passage times is usually done using Laplace transforms. One can verify by integration the classical result for Weiner-Einstein diffusion processes

$$\frac{1}{\sigma\sqrt{2\Pi t}}\exp\{-\frac{d^2}{2\sigma^2 t}\} = \int_0^t \frac{d}{\sigma\tau\sqrt{2\Pi\tau}}\exp\{-\frac{d^2}{2\sigma^2\tau}\}\ \frac{1}{\sigma^2\sqrt{2\Pi(t-\tau)}}$$

$$* \exp\{\frac{-(d-d)^2}{2\sigma^2(t-\tau)}\}d\tau \qquad (1)$$

so the distribution of t, the time for first passage from 0 to d is

$$g(t;d) = \frac{d}{\sigma t\sqrt{2\Pi t}}\exp\{-\frac{d^2}{2\sigma^2 t}\}.$$

Note the expected value of the first passage time does not exist for this density function ruling out regression methods. This density is a maximally skewed stable Paretian density with characteristic exponent one-half.[8] The Weiner-Einstein process is sometimes generalized to allow for drift in the mean over time, for example $E[X(t)] = at$ where $X(0) = 0$. In this case the probability density function for location is

$$f(0,0;x,t) = \frac{1}{\sigma\sqrt{2\Pi t}}\exp\{-\frac{(x-at)^2}{2\sigma^2 t}\} \qquad (2)$$

assuming $X(0) = 0$. In the same way as in (1) it can be verified that the distribution of the first passage time in the case where the mean drifts over time is

$$g(t;d) = \frac{d}{\sigma t\sqrt{2\pi t}}\exp\{-\frac{(d - at)^2}{2\sigma^2 t}\}. \qquad (3)$$

If a is positive the mean of the first passage time is $d/|a|$ and the variance is $d\sigma^2/|a^3|$. .

C. The Covariance between Wage Offers and Labor-Leisure Choice

Let X(t) and Y(t) be two Weiner-Einstein diffusion processes which have started at zero at time zero.[9] Let $t_1 < t_2 < t_3 < t_4$. Assume X(t) and Y(t) have means of 0 and variances $t\sigma_x^2$ and $t\sigma_y^2$ respectively. Therefore X(t) and Y(t) are characterized by the independence of non-overlapping increments; that is $X(t_2) - X(t_1)$ is independent of $Y(t_3) - Y(t_2)$ whereas $X(t_2) - X(t_1)$ and $Y(t_2) - Y(t_1)$ have correlation ρ. Being Weiner-Einstein processes both X(t) and Y(t) have independent increments, so that $X(t_3) - X(t_2)$ is independent of $X(t_2) - X(t_1)$. These processes satisfy the Markov property that

$$\Pr\{X(t_r)=x_r, Y(t_r)=y_r | X(t_1)=x_1, Y(t_1)=y_1; X(t_2)=x_2, Y(t_2)=y_2; \;.\;.\;.\;.$$

$$X(t_{r-1})=x_{r-1}, Y(t_{r-1})=y_{r-1}\} = P_r\{X(t_r)=x_r, Y(t_r)=y_r | X(t_{r-1})=x_{r-1}, Y(t_{r-1})=y_{r-1}\}$$

so the last observation contains all the information there is in the past history of the variable.[11]

The joint density function for X(t) and Y(t) is (using x(t) and y(t) to indicate realizations of X(t) and Y(t), respectively).

$$\frac{1}{2\,t\sigma_x\sigma_y\sqrt{1 - \rho^2 t}}\exp\{-\frac{1}{2(1-\rho^2)}[\frac{x(t)^2}{\sigma_x^2 t} + \frac{y(t)^2}{\sigma_y^2 t} - 2\rho\frac{x(t)\,y(t)}{\sigma_x\sigma_y t}]\}.$$

If we could condition upon the entire history of $y(t)$, the distribution of $x(t)$ given $y(t)$ would be

$$\frac{1}{\sigma_x\sqrt{2\Pi t(1-\rho^2)}}\exp\{-\frac{1}{2(1-\rho^2)\sigma_x^2 t}[x(t)-\frac{\rho\sigma_x}{\sigma_y}y(t)]^2\}.$$

If we had the entire history of wage rates $Y(t)$ for all t the specification of this distribution would have to consider $Y(t)$ at each instant, a heavy burden. Some simplification is provided if we are considering the behavior of $X(t)$ between $t=t_2$ and $t=t_3$ and the wage rate is observed at times t_1 and t_4 where $t_1 < t_2 < t_3 < t_4$. Consider the increment $X(t)-X(t_2)$; $t_2 < t < t_3$. The joint distribution of $X(t)-X(t_2)$ and $Y(t_4)-Y(t_1)$ will be normal, and the covariance between the two increments will be $\rho\sigma_x\sigma_y(t-t_2)$ since the length of the overlapping part of the two intervals is $(t-t_2)$. The correlation between $X(t)-X(t_2)$ and $Y(t_4)-Y(t_1)$ is

$$\rho\sqrt{\pi(t-t_2)/(t_4-t_4)}$$

and

$$E\{X(t)-X(t_2)|Y(t_4)-Y(t_1)\}=\rho\sigma_x(t-t_2)(Y(t_4)-Y(t_1))/\sigma_y(t_4-t_1).$$

The derivation of exact distributions of first passage times for the $X(t)$ series conditional upon observations on Y at t_1 and t_4 is made extremely difficult because the conditional distribution of $X(t)$ given $Y(t_4)-Y(t_1)$ depends upon t in a rather complex way. The distribution of first passage times for time homogeneous diffusion processes[12] is often derived by applying the Laplace transform inversion formula to the quotient of two Laplace transforms of the distributions of the density functions of the diffusion process under investigation (See Barrucha-Reid for detail). Unless the density of $X(t)$ given $Y(t_4)-Y(t_1)$ is fairly simple, it may be virtually impossible to obtain an analytic expression for the first passage time distribution. Two features of this problem complicate matters. First, because of the property of independence of nonoverlapping intervals, $X(t)$ will not depend upon $Y(t_4) - Y(t_1)$ except when $t_1 < t < t_4$. This difficulty can be easily accomodated by only considering the distribution of first passage times for the closed interval $[t_2, t_4]$ where t_2 is the start of an employment (or nonemployment spell) with $t_1 < t_2 < t_4$. As noted above, the mean of $X(t)$ given $Y(t_4) - Y(t_1)$ will be linear in $t-t_2$.

Second, because of data limitations we must always condition upon changes in Y over particular intervals rather than conditioning upon a continuum of values for Y, the variance of the $X(t)$ series starting at t_2 given $Y(t_4) - Y(t_1)$ is $\sigma_X^2[1-\rho^2(t-t_2)/(t_4-t_1)](t-t_2)$. If we were conditioning upon a continuum of values for Y the conditional variance of $X(t)$ series starting at t_2 would be $\sigma_X^2(1-\rho^2)(t-t_2)$. If the variance of $X(t)$ given $Y(t_4) - Y(t_1)$ is linear in $t-t_2$ and the mean of $X(t)$ given $Y(t_4) - Y(t_1)$ is linear in $t-t_2$ then distribution of first passage times for $X(t)$ will be the distribution (3) where $a = \rho\sigma_X[Y(t_4)-Y(t_1)]/\sigma_y(t_4-t_1)$ and $\sigma = \sigma_X^2(1-\rho^2)$, and the normalization $t_2 = 0$ has been adopted. In order to obtain a first passage distribution we have ignored the heteroscedasticity in the diffusion process for $X(t)$ given $Y(t_4)-Y(t_1)$ since the distribution of X is time inhomogeneous and there is no convenient way to derive the first passage time distribution. In effect we have assumed the wage path follows a straight line connecting the observed wages in order to have closed form expressions for our first passage time distributions. We only condition the first passage time density upon the wage path when there are observations on wage rates both before and after the completion of a spell.

D. Wage Growth and its Impact on First Passage Times

One feature of wage profiles is that they are upward sloping over most of the life cycle, reflecting continuing investment in human capital. For simplicity we will assume the wage profile is linear over the ages we consider and that the slope of the profile is the same regardless of whether the person is working or not. The acquisition of human capital may be just as costly when not employed. Unless a spell of nonemployment signals a predisposition to future nonemployment, being out of work need not affect the optimal accumulation of human capital if being out of work does not affect the cost of such accumulation.

We will estimate the diffusion process for wages allowing an upward drift in the mean over time reflecting general human capital accumulation. The process for wages will be, as in (2)

$$f(Y(t_0),t_0;Y(t_1),t_1) = \frac{1}{\sigma_y\sqrt{2\Pi(t_1-t_0)}}\exp\{-\frac{1}{2\sigma_y^2 t}[Y(t_1)-Y(t_0)-a(t_1-t_0)]^2\} \quad (4)$$

which is the transition frequency function at time t associated with a value of the wage rate of $Y(t)$ given the process started at $Y(t_0)$ at time t_0. The expression in (4) describes innovations in the wage process, hence it contains the main feature of a fixed effects model, namely, the differencing of the data. a is the rate of drift over time in the wage rate diffusion process.

The distribution of the initial observation $Y(t_0)$ will have a different form since it is not conditional on an earlier observation. We will simply assume the distribution of $Y(t_0)$ is normal with mean μ_0 and variance σ_0^2. The presence of a time drift in the mean of the diffusion process for the wage residuals has an important implication for the probit-like diffusion process for working versus not working. Suppose a person stops working at time zero. We know the wage offer Y equals (or is a very small amount below) the value of time V at time zero. At time zero we know $Y(0) = \mu_1+e(0)=\mu_2+u(0)=V(0)$ and the duration of this non-employment spell is the time required for $X(t) = Y(t)-V(t) = \mu_1-\mu_2+e(t)-u(t)$ to reach k. This is equivalent to the time it takes $x(t)=e(t)-u(t)$ to transit from $\mu_2-\mu_1$ to $\mu_2-\mu_1+k$. Because we do not directly observe the realized values for the series $x(t)$, but only the times at which the series crosses thresholds whose values must be estimated, the infinitesimal variance of $x(t)$ is not identified, just as the variance of the error term of standard cross-section probit models is not identified. The presence of time drift in Y (the wage process) allows us to identify the infinitesimal variance of Y-V. If the time drift of the $Y(t)$ process is a^*, and we find a time drift in the $X(t)$ process of a^0 and there is no drift in $V(t)$, it can only be that the $X(t)$ process we have estimated is in fact $X(t)=[\frac{a^0}{a^*} Y(t)-V(t)]$. The estimation strategy is to impose the normalization that the infinitesimal variance of $X(t)$ is one, and then based upon the time drift for the $X(t)$ process, proceed to estimate a^0 and so undo the arbitrary normalization. The true threshold value k can be similarly obtained from the estimated threshold value k^0, which we obtain under the normalization that the infinitesimal variance of $X(t)$ is one, by simply undoing the normalization with $k^0 = ka^*/a^0$.

Note that if there is an upward time drift in the wage rate error term, this would induce an upward drift in the X(t) diffusion process. This asymmetry makes the duration of spells of nonemployment shorter than spells of employment. In fact, with an upward drift in X(t) there is finite probability that employment spells will never end.

If X(t) and Y(t) are not correlated the relative lengths of spells of employment and unemployment will not be influenced by whether the person happens to be in a period for which wages are increasing rather than decreasing. If a simple, static selectivity bias model produces the result that low wages tend not to be observed because the person is less likely to be working, then when wages are falling because of the behavior of the random component e(t), the probability the person will be observed working should also be falling. The method suggested here allows us to be more complete in testing for selection bias. The added information contributed by longitudinal data is important to consider since conventional selection bias models applied to cross-sectional data with one observation per person tend not to be very robust.

Consider the behavior of the x(t) series between t_2 and t_3 where $t_1 < t_2 < t_3 < t_4$, conditioning upon the behavior of the Y(t) series between t_1 and t_4. The random component to the Y(t) series is $Y(t_4)-Y(t_1)-a(t_4-t_1)$. Given the change in the Y(t) series, the expected drift in the X(t) has two components. First, X(t) has time drift because Y(t) is a constituent part of X(t), and Y(t) has drift. Second, if the random drift component of Y(t) (that is, the realized drift net of the systematic rate of drift a) is correlated with the random drift component of X(t), conditioning upon the change in Y(t) affects the conditional expectation of rate of drift in X(t). In particular, the expected rate of drift between t_2 and t_3 will be

$$-a^{0}+\rho[Y(t_4)-Y(t_1)-a(t_4-t_1)]/\sigma_y\sqrt{t_4-t_1}.$$

The variance in the normalized X(t) process will be $1-\rho^2$ instead of one where we condition upon $Y(t_4)-Y(t_1)$.

E. Incomplete Spells

Incomplete spells arise in the data for two basic reasons. Because of the design of the questionnaire, certain spells of employment or nonemployment are revealed to be at least of a certain length. Their exact length cannot be determined because either the spell had not ended as of the interview data, or because the starting date of the first spell was not determined in the initial interview even though ancillary information allows us to infer a lower bound on spell length.

These two reasons for incomplete spells can require different treatment. Recall that a Weiner diffusion process which starts at zero and has an upward drift may never cross below some arbitrary negative number. In such an event the interval will remain open no matter how long one waits. An interval which is open only because the starting date of the spell is unknown is different from a right censored spell because in the former case we know the spell actually ended, but in the latter case we do not know whether the spell will ever end. This subtle difference is only important when the diffusion process starts out with a linear trend away from the barrier. If this is the case, the contribution to the likelihood of left censored spells whose observed length is t is $1-\Pr(T<t)-\Pr$ (interval never closes). When the diffusion process has no drift or drifts toward the barrier P (interval never closes) = 0, and left and right censored spells may be treated in the same manner.

F. Duration Dependence

The first passage time density in (3) has a long pedigree in statistics, but may not be familiar to most economists. Waiting time densities are often characterized by describing their hazard functions. The hazard function is defined as

$$\phi(t) = \frac{f(t)}{\int_t^0 f(\tau)d\tau}$$

where f(t) is the probability density function (assumed continuous). The density function can be expressed as

$$f(t) = \phi(t)\exp\{-\int_0^t \phi(\tau)d\tau\}$$

so the density and hazard are dual to one another. Whether one works with the density or hazard is largely a matter of convenience.

The hazard function for the Weiner-Einstein first passage time distribution is

$$\frac{dt^{-3/2}\exp\{-d^2/(2\sigma^2 t)\} \;/\; (2\Pi\sigma^2)^{1/2}}{2F_n[d/(\sigma\sqrt{t})]-1}$$

where F_n is the cumulative normal distribution function. When the diffusion process is subject to drift, the hazard function will be different. However, regardless of whether the diffusion process has drift or not, and regardless of whether the drift is towards or away from the barrier, the hazard has the same behavior. In particular, the hazard briefly increases with duration and then slowly falls. Because the path of a normal diffusion process is continuous, the property of "forgetfulness," which exponential duration models possess, will not hold for the diffusion first passage time distribution.

A passage to the barrier in the first short interval of time, ΔT is unlikely since the variance of the process will be very small (being proportional to elapsed time) and the barrier will be many standard deviations from the starting point to the process. As more time passes, the barrier will be fewer standard deviations from the unconditional mean of the process, which will tend to increase the probability of a passage, and hence at least initially increase the hazard. Once the process has been operating for a long time, the variance will be so large that the barrier will be, for all practical purposes, at the unconditional mean of the process.[13]

If the process has been operating for a long time without having crossed the barrier, it is likely the process is located relatively far from the barrier and hence it is less likely that a passage will occur in the next small interval of time. While the process is characterized by independent behavior in nonoverlapping intervals, the location of the process will have strong dependency over time and it is this dependence which generates a declining rather than a constant hazard.

Note that despite the essentially static nature of the model and the lack of a heterogeneity component specific to the individual, the distributions of waiting times are characterized by duration dependence.

III. THE DATA[14]

The data are from the youth sample of the National Longitudinal Surveys (NLS). Responses to labor supply questions have been organized to indicate whether during each week covered by the survey (1979-1983) the youth were employed or not.

Questions on wage rates were such that the information on wage rates was obtained either for the week of an interview or when a job spell ended. Youths with missing values for exogenous variables were dropped from the sample. Spells in the military were dropped. The sample has data on 8,279 persons, and contains 95,222 data points representing the activity at the start of the survey, transitions in and out of unemployment, and observed changes in the wage rate. Wages are measured in natural logs. Respondents with wages which were absurd (wages above $50 or below $.50 per hour) were dropped. Descriptive statistics are presented in Table 3.1.

IV. EMPIRICAL RESULTS

The central result of the estimation is the (apparent) finding that there is no selectivity bias in the estimation of the wage offer equation when longitudinal data is used with the method described above. This result is in stark contrast to what is found using only cross-sectional data at the first interview. Using the method of maximum likelihood we are led to find that the population mean of the wage offer distribution is underestimated using least squares. This result implies that in the cross-section those persons with unusually high wage rates are not employed. This would occur when there is a large positive correlation between the value of time and the market wage, and the standard error of the value of time equation is larger than the standard error of the wage offer equation. It is this latter case which produces the counter-intuitive result that selection bias causes the mean wage to be understated. Concommitant with the powerful effects of selection upon the estimated population mean, the various wage coefficients likewise undergo substantial change and occasional sign reversal. Identification in the cross-sectional selection exercise is achieved by the exclusion of variables for

Table 3.1 Descriptive Statistics

Variable	Sample Mean
College prep. program	0.30571
Vocational program	0.15968
Education	10.499
Father's education	9.5252
Mother's education	11.016
Black	0.23324
Hispanic	0.12671
North-East	0.18529
North-Central	0.26416
South	0.37106
SMSA	0.74176
Unemployment rate	8.0550
Male	0.49281
Health impaired	0.058461
Protestant	0.55284
Catholic	0.31779
Father white collar worker	0.25728
Father blue collar worker	0.48798
Mother white collar worker	0.23421
Mother blue collar worker	0.28313
Married	0.080444
Child under 6	0.11040

occupation of the mother and father, marital status, and presence of a child under six from the wage equation. The assumption of normality also plays a role in identification.

In the longitudinal estimate, we check whether fluctuations in the wage offer and in the value of time are as strongly correlated over time as they appear to be in a single cross-section. The answer appears to be no. The infinitesimal variance of the value of time process is larger than the infinitesimal variance of the wage offer process, but the positive correlation between the two is of such a magnitude that selectivity bias is not present.[15]

The place to begin summarizing the results from the diffusion model is with the wage offer/participation model for the initial observation. The results here are quite simply the results from the simple bivariate normal selection model with essentially independence between the probit model and the wage offer equation. The wage offer and value of time processes then follow a normal diffusion process starting from the initial point.

The wage offer increases at 0.232 percent per week. The infinitesimal variance of the wage offer process is about .0053. The value of time is assumed not to drift (an identifying assumption) and has an infinitesimal variance of about 0.044. These two diffusion processes covary positively, the correlation being 0.14.

The value of d, which is the amount above the value of time the wage must reach to induce a nonworker to begin a spell of employment, is about 0.52. The wage offer must be 68 percent above the value of time in order to induce a nonworker to work. Note there is no unemployment or search in this model.

Table 3.2 compares the estimated wage equation from a single cross-section using the maximum likelihood method of dealing with selection bias with the results obtained using ordinary least squares without any correction

Table 3.2 Comparison of Wage Equation Estimates

Variable	Maximum Likelihood	Least Squares
Population mean	1.697	1.047
	(167.4)	(107.1)
College prep. program	-0.0257	-0.0615
	(0.97)	(2.67)
Vocational program	-0.0325	0.0159
	(1.08)	(.618)
Education	0.0507	0.0747
	(8.69)	(14.8)
Father's education	0.000998	-0.0153
	(0.45)	(0.78)
Mother's education	-0.000678	-0.00584
	(1.51)	(1.49)
Black	0.157	0.0766
	(5.07)	(2.82)
Hispanic	0.0312	-0.0334
	(0.75)	(0.93)
North-East	-0.0651	-0.0807
	(1.74)	(2.45)
North-Central	-.151	-.106
	(4.39)	(3.57)
South	-0.0756	-0.0764
	(2.24)	(2.51)
SMSA	0.0331	0.0334
	(1.26)	(1.42)
Unemployment rate	0.00528	0.00203
	(1.16)	(0.50)
Male	0.129	0.181
	(5.87)	(9.35)
Health impaired	-0.0395	-0.0458
	(0.83)	(1.00)
Protestant	-0.0373	-0.0562
	(1.12)	(1.90)

Table 3.2 (continued)

Variable	Maximum Likelihood	Least Squares
Catholic	-0.00333 (0.09)	0.0102 (0.32)
Standard error	0.348 (42.16)	0.413 (13.1)
Selection correlation	0.714 (8.02)	--

for selection. The estimated t-statistics are in parenthese below each coefficient. Because the model based upon longitudinal data does not reveal any selection bias, its coefficients agree with the ordinary least squares results.

Note there is a difference of over 60 percent between the mean wage offer estimated using the cross-sectional maximum likelihood method and the mean wage offer obtained using the full longitudinal data (the later being identical to the least squares results). In addition, the coefficients of race, education and sex differ markedly between the two sets of estimates. These large differences illustrate the importance of econometric method to one's findings on labor market outcomes.

V. CONCLUSIONS

Sample selection models are notorious for generating strange results and many empirical researchers have been suspicious of selection models. The proper response to such scepticism is not to dismiss the selection model, but to conduct empirical work so that the selection model generates a number of implications for the data which can be used as diagnostic checks.

The model here is simply an extension of the static model to longitudinal data. However, even this very simple model, when coupled with the efficiency of longitudinal data, is enough to reverse a rather strong conclusion from the cross-section. The method used here does not explicitly introduce fixed effects, but by focusing upon changes in wage rates and work status some of the confounding effects of unobservables are removed.

Chapter 3

Notes

[1] For example, the work on effectiveness of public versus private schools by Coleman, Hoffer and Kilgore (1981) was reanalyzed by Noell (1982) using the Heckman (1976) two-step estimator of the sample selection model. Noell reported that, using the selection estimator, no difference was found between public and private schools, in contrast to the Coleman et al. findings. While other differences in the analysis do play a role in the different findings, this is simply one example of how the use of a sample selection method has had a large effect upon the substantive conclusions drawn from empirical work.

[2] In the early literature, it was typically assumed the error terms of the wage offer equation and the participation model were bivariate normal. Lee (1982) discusses a variety of ways to relax normality in the participation model and their implications for two-step methods. Olsen (1982) discusses methods of relaxing normality in the wage offer model and their implications for maximum likelihood methods.

[3] Because diffusion models based upon normal processes are continuous, the highest wage offer will be non-zero. If we observe wage rates of $6 an hour and $5 an hour 10 weeks apart, there is virtually no chance that the highest wage rate between these two times is ever close to zero. If a person does not work in the period between these two observations, then according to this model, it can only be because the reservation wage has moved up relative to the wage offer so the person chooses not to work. There is no involuntary unemployment in this model, hence it should probably not be considered part of that research genre.

[4] Examples can be found in Lippman and McCall (1976), who consider a variety of models, and Flinn and Heckman (1982).

[5] This assumes there is no drift in the wage offer over time, that is, no wage growth due to investment. Below we will relax this assumption to allow the wage process to have constant drift reflecting such investment.

[6] This is a consequence of the normality assumption. If the increments were independent, identically distributed, stable Paretian with characteristic exponents under two, then there would be discontinuous jumps in the process. See McCulloch (1984).

[7] Because the underlying stochastic process is normal, the path taken by the process is continuous but not differentiable. If the stochastic process were based upon a stable Paretian distribution with characteristic exponent less than two jumps could occur, compounding the difficulties of determining the first passage time distribution. See Bharucha-Reid (1960) for a discussion of the first passage time problem.

[8] See Feller (1966), p. 171.

[9] We will interpret $X(t)$ to be the difference between the value of time at full-time "leisure" and the best wage offer the person can get at that instant in the market, so $X(t) = Y(t)-V(t)$. $X(t)$ plays the role of the probit index in the conventional one period labor-leisure model.

[11] Note that while the underlying stochastic processes are Markovian, the distributions of durations generated (see equation (3)) is not exponential.

[12] Time homogeneity requires $f(x_0,s;x,t) = f(x_0,s+r;x,t+r)$. This does not hold for the model here unless we assume the wage process upon which we are conditioning follows a straight line between sample points.

[13] This assumes the diffusion process has no drift. If the process drifts away from the barrier, the closest the unconditional mean comes to the barrier measured in standard deviations of the process is at $t=d/|a|$ where d is the distance from the starting point of the process to the barrier and a is the rate of drift in the mean of the process. If the process drifts towards the barrier first passage times tend to be much shorter. Only in the case of drift towards the barrier does the first passage time have a finite mean.

[14] I am indebted to Yu-Hsia Chen for helping me prepare the data for analysis and to Dennis Grey for advice and assistance.

[15] In order for selection bias to be absent in cross-sectional data, the value of time and wage offers must be positively correlated. If the two are uncorrelated, selection will be present with the sample mean of wage offers below the population mean.

Chapter 3

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Chapter 4

High Youth Unemployment in the 1970's: Its Effect in the '80s

by

Stephen M. Hills

High rates of youth unemployment in the 1970s resulted in significant legislative changes during the Carter administration. Youth age 14-21 were eligible for training or subsidized employment programs which were established under the Youth Employment Demonstration and Projects Act of 1977 and administered through the Department of Labor's Office of Youth Programs. In 1978-79, 2.4 million youth under the age of 22 were enrolled in federal employment programs, three-fifths in programs which provided special work experience (mainly summer programs for youth on vacation from school) and another 13.5 percent in created jobs or subsidized employment (GAO 1982, p. 41). The intervention of the federal government into youth labor markets undoubtedly reduced youth unemployment in the short run since as many as 6 percent of youth age 15-21 participated in at least one program in a given calendar year (Crowley, 1981). But was the intervention required to lessen the long-run impact of unemployment?

A careful reading of previous research yields a somewhat uncertain answer to the above question. Ellwood (1982), Heckman and Borjas (1980), Corcoran (1982), Meyer and Wise (1979), Osterman (1980), and Stevenson (1978) all have found negative effects of early unemployment on subsequent earnings. But Becker and Hills (1980, 1983a, 1983b) found relatively little "scarring" associated with teen unemployment, and in fact argued that brief periods of unemployment, if coupled with job mobility, might even result in better long-run earnings possibilities, particularly for blacks.

A single problem limits the applicability of these studies to the unemployment experienced by youth during the 1970s. Without exception, all of them were based on youth labor market experience which occurred in the late 1960s and early 1970s. If the unemployment of the late 1970s differed significantly from that which occurred in the 1960s or the 1970s, then conclusions based on the earlier period could be misleading. Pollard (1982) argues that indeed the unemployment of the earlier time period was different, especially for blacks. According to Pollard, black unemployment in the late 1960s was characterized by a high degree of turnover. In contrast, long spells of unemployment, uninterrupted by periods of work, typified black unemployment in the late 1970s and early 1980s.

Researchers concentrated their attention on a period of tight labor markets because longitudinal data were available. The National Longitudinal Surveys (NLS) were initiated for men age 14-24 in 1966 and for women age 14-24 in 1968. The studies by Becker and Hills, Ellwood, Heckman and Borjas, Osterman and Stevenson were all based on NLS data. The Panel Study of Income Dynamics, initiated in 1968, contained a large enough sample of young women to allow Corcoran to perform her analysis on these data. Finally, the High School Class of '72, initiated in 1972, provided the data for the study by Meyer and Wise.

Only now have new longitudinal data become available to test the potential relationship between unemployment experienced in the late 1970s and subsequent labor market outcomes. The NLS new youth cohort, initiated in 1979, has accumulated a sufficiently long record of information to test for a relationship between early unemployment and subsequent earnings.

In this study, a 12 month period from spring 1979 to spring 1980 was used for collecting data on the early unemployment experienced by young men and women when they were ages 15-22. We asked if the unemployment that young people experienced from 1979 to 1980 had an impact on their earnings in 1984. The period from 1979-1984 was quite different from the period when earlier studies were done on the effects of teen unemployment. CPS data show that in 1979 the unemployment rate for youth age 16-19 stood at 16.1 percent. The rate rose to 23.2 percent by 1982 and fell to 18.9 percent in 1984. In 1968, the unemployment rate for youth age 16-19 was only 12.7 percent. By 1971 it had risen to 16.9 percent but fell again to 14.5 percent by 1973. Thus, rates were 3 to 6 percentage points higher in the 1980s than they were in the late 1960s and early 1970s. The differences between the two time periods were even more pronounced for blacks (see Table 4.1).

I. MODELING THE RELATIONSHIP BETWEEN EARLY UNEMPLOYMENT AND SUBSEQUENT LABOR MARKET OUTCOMES

The labor market outcome examined here is earnings. Intervention of the federal government into youth labor markets could be defended either on the basis of the short-term effects of youth unemployment on current incomes, delinguency, etc., or the possible long-term effects on employment stability or earnings. Our task is to determine: 1) whether high levels of youth unemployment have negative long-run effects on earnings; and 2) whether unemployment itself or the lack of work experience is more related to potential earnings losses.

Understanding the relationship between youth unemployment and the total amount of time a youth has worked is an important prerequisite for modeling the effects of early unemployment on subsequent earnings. Early unemployment is part of the total work experience that a youth carries through time. Work experience contributes to the general skills that a person will use throughout a work career and thus enhances earnings. If early unemployment reduces work

Table 4.1 Unemployment Rates for Youth Age 16-19, by Sex and Race, 1968 to 1973 and 1980 to 1984

		Total		Males		Females	
Year	Total	Male	Female	White	Black & Other	White	Black & Other
1968	12.7	11.6	14.0	10.1	22.1	12.1	28.7
1969	12.2	11.4	13.3	10.0	21.4	11.5	27.6
1970	15.3	15.0	15.6	13.7	25.0	13.4	34.5
1971	16.9	16.6	17.2	15.1	28.8	15.1	38.4
1972	16.2	15.9	16.7	14.2	29.7	14.2	34.4
1973	14.5	13.9	15.3	12.3	26.9	13.0	34.5
1979	16.1	15.9	16.4	13.9	31.3	14.0	35.6
1980	17.8	18.3	17.2	16.2	34.4	14.8	36.5
1981	19.6	20.1	19.0	17.9	37.5	16.6	38.3
1982	23.2	24.4	21.9	21.7	44.0	19.0	43.8
1983	22.4	23.3	21.3	20.2	45.0	18.3	44.6
1984	18.9	19.6	18.0	16.8	38.9	15.2	38.5

Source: U.S. Department of Labor, Bureau of Labor Statistics; Labor Force Statistics Derived from the Current Population Survey: A Databook. Vol. 1. Bulletin 2096, Table A-28, 1982. Data from 1982-1984 are reported from January issues of U.S. Department of Labor, Bureau of Labor Statistics, Employment and Earnings.

experience, it will in turn reduce subsequent earnings. But does the literature on the effects of youth unemployment refer only to the reduction in earnings caused by reduced work experience? An earnings model usually includes a measure of total work experience as one of the independent variables. If the only effect of youth unemployment is to reduce work experience, then in an earnings equation it should be strongly correlated with total work experience. Furthermore, a significant relationship between early unemployment and earnings might simply imply that total work experience was poorly measured and unemployment served as a good proxy for joblessness in the early years of a work career.

A more important hypothesis from the literature on the "scarring" effects of early unemployment is that an unexpectedly high rate of unemployment early in one's working life will leave an indelible mark--either on the person's motivation to seek out decent employment or on a person's work record such that employers are less willing to hire a youth who has experienced large amounts of unemployment. The literature on scarring carries with it less implication that early unemployment reduces work experience and therefore, by definition, reduces long-run earnings. In our earnings equation, therefore, we must take care to measure total work experience in the best way possible.

In the 1979 NLS interview, respondents were asked to report retrospectively the weeks they had worked since age 18. In the analysis which follows, we construct a measure of total work experience, adding to the 1979 report of weeks worked since age 18 the total weeks of work that respondents report in each subsequent survey. This approach yields a more complete definition of total work experience than has generally been used in earnings equations.[1]

Understanding the relationship between youth unemployment and job mobility is also important when modeling the long-run effects of youth unemployment. Job mobility can be an indication of job search which, if successful, leads to greater earnings and employment stability, or job mobility can be a sign that youth are trapped in a series of dead-end jobs with high turnover and little chance for advancement. In either case, the mobility may or may not accompany spells of unemployment. To measure the effects of job mobility, we identify youth who have changed jobs during the 1979-1980 period of time. We distinguish between youth who have job mobility accompanied by unemployment and youth who experience job mobility and no unemployment. Each of these groups is compared with youth who do not change jobs during the 12-month period. We thus test to see if the effects of job mobility derived from the labor market experience of youth in the late 1960s and early 1970s (Becker and Hills, 1983a) is replicated for a new generation of youth.

Researchers have used a variety of specifications for early employment. The simplest (though probably least desirable) measure was used by Stevenson--a youth's employment status at a given point in time during the teen years. The single-valued variable--employed, unemployed--is apt to capture a variety of characteristics about an individual which are hard to separate from the unemployment experience. A more commonly used measure is the number of weeks of unemployment that a youth experiences over a specified period of time. A calendar time period can be established by selecting boundary dates and examining the unemployment which youth experience between these dates. Alternatively, a time period can be fixed with regard to life cycle events: for example, the transition period immediately after leaving school. In our analysis, we have used both approaches. First we measured the unemployment that all out-of-school youth who were not in the military experienced from the spring of 1979 to the spring of 1980. Next we selected the subset of youth who had left school in the 12-month period from spring 1978, and measured their unemployment from the spring of 1979 to the spring of 1980. For the larger sample and for the subsample, we estimated the relationship between the unemployment experienced from 1979 to 1980 and the wages earned in 1984, holding constant total work experience.

Differences in labor force behavior which may exist between men and women further complicate the modeling of the long-run effects of early unemployment. In our modeling we treat men and women alike, using the same set of variables to predict earnings. We require that both men and women be out of school and not in the military from 1979 to 1980, and that both groups be employed and receive wage and salary income in 1984. Because of the employment restriction, men and women with marginal attachment to the labor force in 1984 are excluded from the analysis. Youth who remain in the sample should therefore show strong attachment to the labor force between 1979 and 1984 as well, but this was not always the case. We found, for example, that 2 percent of the white men, 5 percent of the minority men, 11 percent of the white women, and 20 percent of the minority women who remained in our sample were not in the labor force for even one week from 1979 to 1980 and could not be classified as discouraged workers during that time period either.

A youth's choice to participate in the labor force could be incorporated in the model which relates early unemployment to subsequent earnings. To simplify the problem, however, we estimate the relationship between early unemployment and subsequent changes given that a young person has expressed a preference for labor market activity, either by participating in the labor force for at least one week in the 1979-1980 time period or by indicating that the reason for non-participation was a belief that no jobs were available (discouraged worker). Though it introduces potential sample selection bias, this procedure permits a more parallel analysis of young men and women.

We followed two strategies to test the sensitivities of our model to the sample selection rules we used. First, in some models we added the amount of time spent out of the labor force (OLF time) from 1979 to 1980 as one of the independent variables for predicting 1984 earnings. Thus we could determine whether the relationship was changed once OLF time was held constant. Second, we made parallel estimates for a sample of youth who were not restricted in terms of their labor force participation from 1979-1980.

II. RESULTS

The OLS estimates by race and sex for 1984 earnings equations are shown in Tables 4.2-4.4. Wages are expressed in logarithmic form, reflecting the presumed non-linear nature of the earnings function when related to key human capital characteristics like education, work experience, and training. When expressed in this form, a regression coefficient can be interpreted approximately as the percentage increase in the wage rate which is associated with a one unit change in any one of the independent variables.[2] Early unemployment is expressed both in a linear form (the variable "Unemployment Duration" shown in Table 4.2) and in a non-linear form (the variables "Unemployment Duration" and "Unemployment Duration Squared" shown in Table 4.4). The linear specification has been used most often in prior research, but the non-linear form may be more appropriate if, under some circumstnaces, early unemployment is linked to a decrease in long run earnings, while under others, it is linked to increased subsequent earnings.

Table 4.2 Regression Analysis of the Effects of Early Unemployment on 1984 Hourly Earnings, Youth in the Labor Force in 1980, by Sex and Race

(t-values in parentheses)

Dependent Variable: Natural Log of Hourly Wage	White		Minority	
	Males	Females	Males	Females
Independent Variables:				
Constant	5.893 (29.73)	5.125 (22.70)	5.301 (26.69)	5.835 (22.02)
Years of Education	-.020 (-1.13)	.051 (2.64)	.065 (4.16)	.023 (1.03)
High School Degree	.242 (3.87)	-.127 (-1.88)	.049 (-.76)	-.013 (-.15)
Weeks of Training	.0002 (2.28)	.000 (.51)	.000 (1.00)	.000 (1.09)
Weeks of Work Experience Since 18	.001 (3.66)	.002 (6.59)	.001 (3.14)	.001 (2.57)
Weeks of Tenure	.005 (4.83)	.005 (5.11)	.004 (2.18)	.005 (3.86)
Married	.089 (2.54)	-.049 (-0.49)	.056 (.056)	-.038 (-.038)
Hispanic			.118 (2.15)	-.062 (-.99)
Locus of Control	.000 (.02)	-.009 (-1.20)	-.006 (-.65)	-.017 (-1.52)
Union	.262 (6.34)	.218 (3.85)	.229 (4.53)	.314 (4.97)
Resides in Northeast in 1984	-.026 (-.53)	.089 (1.72)	.120 (1.65)	.130 (1.57)
Resides in North Central in 1984	-.086 (-1.94)	-.066 (-1.42)	.111 (1.54)	-.067 (-.82)
Resides in West in 1984	.187 (3.50)	.131 (2.41)	.165 (.254)	.131 (1.77)
Area Unemployment Rate in 1980	-.002 (-2.19)	-.000 (-.43)	-.002 (-1.77)	-.003 (-2.41)
Labor Force Size, 1980	.077 (3.62)	.074 (2.77)	.010 (.53)	-.021 (-.89)
Some Unemployment, and Job Changes 1979	.053 (.92)	.060 (1.02)	.003 (.04)	-.041 (-.52)
No Unemployment, and Job Changes 1979	.183 (2.99)	-.018 (-.37)	-.047 (-.66)	-.030 (-.43)
Unemployment Duration in 1979	.006 (2.02)	-.003 (-.92)	-.005 (-1.59)	-.007 (-1.87)
Sample Size	666	619	428	287
Adjusted R^2	.300	.256	.297	.237
F	17.753	13.533	9.582	5.934

Universe: Youth out of school and not in the military who were in the labor force in 1979 or were classified as discouraged workers in 1979 and who had

Table 4.3 Definitions of Variables Used in Table 4.2

Years of Education:	Number of years of schooling completed as of 1984 interview
High School Degree:	Dummy variable coded 1 if high school degree was obtained as of 1984 interview, 0 otherwise
Weeks of Training:	Total post-secondary training obtained as of the 1984 interview. Excludes on-the-job training
Weeks of Work Experience Since 18	Total weeks of work between age 18 and the 1984 interview
Weeks of Tenure	Weeks since respondent started work for employer of current or last job held in the 12-month period prior to the 1984 interview
Married	Dummy variable coded 1 if married, 0 otherwise
Hispanic	Dummy variable coded 1 if race is Hispanic, 0 otherwise. (Only included in regression equations for minority youth.)
Locus of Control	The sum of the four items from Rotter's "Locus of Control" scale which are initiated with the word "I"
Union	Dummy variable coded 1 if the respondent's wages for the job held in 1984 were set by collective bargaining, 0 otherwise
Resides in Northeast	Dummy variable coded 1 if the respondent's wages for the job held in 1984 were set by collective bargaining, 0 otherwise
Resides in North Central	Dummy variable coded 1 if respondent lived in the north central Census region of the U.S. in 1984, 0 otherwise
Resides in West	Dummy variable coded 1 if respondent lived in the west Census region of the U.S. in 1984, 0 otherwise
Resides in South	Omitted category to which other residence dummy variables are compared
Area Unemployment Rate	The 1980 Census unemployment rate for the county or SMSA in which a respondent resided as of the 1980 interview

Table 4.3 (continued)

Labor Force Size	The 1980 Census size of labor force for the county or SMSA in which a respondent resided as of the 1980 interview
Some Unemployment and Job Changes 1979	Dummy variable coded 1 if respondent had at least one week of unemployment in the 12 month period preceding the 1980 interview and 1 or more job changes, 0 otherwise
No Unemployment and Job Changes 1979	Dummy variable coded 1 if respondent had no weeks of unemployment in the 12 month period preceding the 1980 interview and 1 or more job changes, 0 otherwise
Unemployment Duration	Total weeks of unemployment experienced by the respondent in the 12 month period preceding the 1980 interview

Table 4.4 Partial Regression Results of the Effects of Early Unemployment on Subsequent Wages, with a Non-Linear Specification for Unemployment, by Race and Sex

(t-values in parentheses)

Dependent Variable:	White		Minority	
Natural Log of Hourly Wage	Males	Females	Males	Females
Independent Variables: Same as in Table 4.2 and including				
Unemployment Duration, 1979	.018 (2.03)	-.012 (-1.52)	.0078 (0.84)	.014 (1.41)
Unemployment Duration, 1979, Squared	-.0004 (-1.40)	.0003 (1.26)	-.0004 (-1.42)	-.0006 (-2.29)
Sample Size	666	619	428	287
Adjusted R^2	.300	.256	.256	.237
Joint F test for adding unemployment variables	3.25	1.23	1.17	4.59

The potential for either a negative or a positive relationship between early unemployment and subsequent earnings implies a heterogeneity of youth labor market experience. Youth unemployment may reflect two quite different situations. On the one hand, unemployment may be synonomous with job search which, if productive, should result in improved subsequent wages. On the other hand, unemployment may reflect the waiting time necessary for youth to find jobs in a more or less haphazard way. When a local labor market has high unemployment, the waiting time will be lengthened, and as a consequence, youth who are somewhat marginally attached to the labor force from the outset may become even more discouraged about their future job prospects, dropping out of the labor force altogether or taking poor paying jobs without exploring other possibilities. In this case, early unemployment could show a negative relationship with long-run earnings. Finally, extremely long spells of unemployment may be used by employers to screen prospective employees into a rationed supply of high paying jobs, whether or not the link between individual unemployment and productivity was justified. In such a case, a negative relationship would exist between early unemployment and long run earnings, even after we control for the characteristics of youth which should be related to their long run productivity. Estimates of the linear and non-linear specifications across race and sex sub-groups of youth may demonstrate which of these explanations is more appropriate.

Table 4.2 shows that human capital variables are, in most cases, statistically significant determinants of 1984 hourly pay. With the important exception of minority women, education was strongly linked to pay, with either the high school degree or years of education statistically significant in each equation. Training, though significant for white men, was not a significant determinant of hourly pay for white women or minorities, and even for white men its numerical impact was small. Work experience was a strong factor affecting 1984 pay with effects similar for all but white women. White women's 1984 hourly pay rose by two-tenths of one percent for each additional week of work they accumulated after age 18. For all other youth, a week of work experience raised 1984 hourly pay by one-tenth of a percent. The effect of job tenure likewise did not differ much across race and sex groups, but its effect was considerably stronger than the effect of general work experience. Each week of tenure raised hourly earnings by a half a percent for all but minority men, where it raised earnings by three-tenths of a percent.

Obtaining unionized employment was quite important for our sample of youth. All else constant, a unionized job raised 1984 earnings from 22 to 31 percent, depending on a youth's race and sex.[3] Geographic location was also important. Youth from western states earned 13-19 percent more than youth from the South, but a youth's wage rate in any other region of the country seldom differed significantly from wages prevailing in the South, all else constant.

The results for other control variables can be seen from Table 4.2, but of most interest to us are the variables describing the unemployment and job mobility of youth in 1980. In none of the equations was job mobility, if accompanied by unemployment, either positively or negatively associated with long-run earnings. Job mobility, if not accompanied by unemployment, was associated with a 20 percent increase in the 1984 earnings of white males, but it had no effect for any other race or sex group.

For white males and minority females, the amount of unemployment experienced in 1979 was associated with a significant difference in 1984 earnings, but interestingly enough, the direction of the effect was different for white males than for minority females. Young white men who spent a longer time unemployed in 1980 had higher 1984 earnings. Each week of unemployment was associated with increased earnings of 0.6 percent, whereas for young minority women each week of unemployment in 1979 was associated with decreased 1984 wages of approximately the same amount, 0.7 percent. For white women and minority men, early unemployment was not significantly related to long-run earnings.

The positive relationship which we have found between early unemployment and the 1984 wages of white males (Table 4.2) could reflect the pay-off to more extensive job search. According to our equations, however, the relationship does not stem from any connection between job mobility and earnings. The control variable for mobility accompanied by unemployment was insignificant in the white male equation and in every other equation as well. If job search were the reason for the positive relationship between unemployment and earnings, workers who changed jobs while unemployed should have significantly higher earnings than workers who did not. Because this is not the case, the positive relationship must be due to some other factor. A likely possibility is that youth in jobs which require higher than average rates of unemployment (e.g., construction) also earn more in the long run.

As noted earlier, a negative relationship between unemployment and earnings may occur through the effects of waiting time on individual attitudes. To some degree, a relationship between waiting time and subsequent wages may be captured by the control variable for the local labor market unemployment rate which was significant in all equations and had the expected negative sign, indicating that higher local unemployment rates accompany lower individual wage rates. This result may say little about waiting time but may mean that in labor markets with high unemployment rates, the entire wage scale is low relative to other markets. If, however, the waiting time argument applies most strongly to youth with weak attachment to the labor force, the negative relationship between the unemployment and wages of minority women may not be coincidental. Our sample of minority women was reduced by 20 percent when we excluded respondents who had no attachment to the labor force between 1979 and 1980, but the sample was reduced by far less than for any other race or sex group. A significant negative relationship for minority women but not for any other group is therefore consistent with our argument, even though the evidence is insufficient to prove the argument correct.

If different race and sex groups show inverse relationships between unemployment and subsequent wages, then the effects of unemployment may also vary with the duration of unemployment. For example, if unemployment is of short duration, the positive effects on wages may prevail, while unemployment of long duration may be linked with lower pay. The test for non-linearity between unemployment and earnings is shown in Table 4.3.

The joint F tests show that the addition of the two unemployment variables in the non-linear specification is significant only for white men and minority women. For both groups, the positive sign on the variable "unemployment" indicates that for unemployment of short duration, the relationship with 1984 earnings is positive, but as duration grows, the negative sign on the variable "unemployment squared" signifies that the relationship eventually becomes negative. The values of the coefficients show that the point at which the relationship becomes negative is very different for white men than for minority women, however. White men's unemployment has a negative relationship with 1984 wages only after 45 weeks of unemployment have occurred in a 12 month period. For minority women, each week of unemployment is associated with an increase in 1984 wages when duration is less than 24 weeks. Only when duration is 24 weeks or more does an additional week of unemployment reduce 1984 wages.

When expressed as a non-linear relationship, the influence of unemployment on the subsequent wages of minority women is much different than when the relationship is linear. Women with long durations of unemployment must have had an undue effect in the first model, since a non-linear specification shows that only unemployment of long duration is linked to reductions in long-run earnings. Very long durations of unemployment may proxy for unmeasured characteristics of minority women which have not been controlled for in our model and which in turn may lead to lower long-run earnings. It may also mean that only very long durations of unemployment are sufficient to depress long-run earnings by affecting a woman's attitudes or by "marking" her as a bad risk for hiring into a good paying job.

For white males, the linear and non-linear specifications are more similar since durations of 45 weeks or more in a single year are of little practical significance. Therefore, within a range of unemployment that most men experience, the relationship with long-run earnings is positive. For women, however, unemployment of 24 weeks or more in a single year is of some significance. CPS data show that in 1979, 10 percent of all women with some unemployment during the year experienced a duration of 27 weeks or more. This same group accounted for 2 percent of all women who had participated in the labor force during 1979 (U.S. Department of Labor, 1982, Vol. 1, Table C-4). Nevertheless, our non-linear model implies that the negative relationship between unemployment and subsequent earnings is confined to a rather small group of individuals since they apply only to minority women. According to the CPS, non-white women accounted for just one-fourth of the women who experienced some unemployment in 1979 (U.S. Department of Labor, 1982, Vol. 1, Table C-4).

The non-linear specification has helped to clarify the initial results for the effects of early unemployment on subsequent earnings. However, another explanation is possible for the negative relationship between unemployment and subsequent wages, particularly for women. The unemployment which women experience may proxy for joblessness (i.e., time unemployed plus time out of the labor force). Thus, the lack of critical work experience in 1979 may be what contributes to depressed long-run earnings, particularly if it is correlated with a general pattern of reduced work experience in each year between 1979 and 1984.

To test for this possibility, we reestimated our equations including the number of weeks out of the labor force (OLF) in 1979 as an additional control variable. We expected that this variable would have little effect on the results for men, since they were quite consistently in the labor force between 1979 and 1984. But for women, if a week spent OLF had the same negative effect on subsequent wages as a week spent unemployed, we would conclude that the loss of work experience at that particular point in time was a critical factor in reducing wage rates. If, on the other hand, the OLF variable were insignificant or had a coefficient of quite different magnitude from that associated with unemployment, we could conclude that the two variables had independent effects on wage rates and that unemployment may have affected wage rates through its indirect influence on attitudes toward work.

As can be seen in Table 4.5, the introduction of the additional control variable for time spent out of the labor force did not affect the results for white males. The coefficient for unemployment experienced between 1979 and 1980 remains positive and with approximately the same effect on 1984 wage rates as before. We expected that the control variable would have similar effects for both groups of women, but it had different effects for minority

Table 4.5 Partial Regression Results of the Effect of Early Unemployment on Subsequent Wages, Controlling for Time Out of the Labor Force, by Race and Sex

(t-values in parentheses)

Dependent Variable: Natural Log of Hourly Wage	White Males	 Females	Minority Males	 Females
Independent Variables: Same as in Table 4.2 and including:				
Unemployment Duration, 1979	.007 (2.06)	-.006 (-1.75)	-.005 (-1.64)	-.008 (-2.05)
Weeks OLF, 1979	.001 (.45)	-.004 (-2.53)	-.001 (-.40)	-.002 (-.84)
Sample Size	666	619	428	287
Adjusted R^2	.298	.262	.262	.224

women than for whites. In the case of whites, both time OLF and time unemployed were significantly related to subsequent wages. Furthermore, the numerical effects of both OLF and unemployed time were fairly similar. An additional week of time spent OLF in 1979 reduced 1984 wages by four-tenths of one percent, whereas an additional week of time spent unemployed reduced wages by six-tenths of a percent.

For young women who were white, early unemployment alone did not have a significant impact on subsequent earnings. Only after time OLF was also controlled, did early unemployment have an effect, and the size of the effect was not very different from the effect of time OLF on long run earnings. We infer, then, that the loss of work experience (i.e., the sum of time unemployed and time OLF) in 1979 was the key factor in reducing 1984 wages. For minority women, in contrast, the amount of time spent OLF did not have an independent effect on 1984 earnings, but the amount of unemployment did.

To test our results for sensitivity to our sample selection rules (which limited the sample to youth who had some form of attachment to the labor force in 1979), we reestimated our equations for all youth out of school and not in the military in 1979. The pattern of results (though not shown here) was very similar to those in Table 4.4, even though variables measuring time unemployed and time out of the labor force showed weaker statistical significance in the regression equations.

Our results do not lend strong support to the contention that early unemployment has a depressing effect on subsequent earnings over and beyond the loss in earnings that could be linked to a loss of work experience. For white males, in fact, early unemployment was linked with increased longer-run earnings. For minority women, the negative effect which appeared with a linear specification between unemployment and earnings became largely positive with a non-linear specification. For white females and for minority males, we could not isolate a statistically significant relationship between early unemployment and subsequent earnings.

But what can we say about the importance of employment in the early years after leaving school? Does joblessness during these years impose a heavy cost on youth a number of years later? Is the cost of losing 6 months of work experience early in one's career more serious than the cost would be, on average, over the first 10 years after leaving school? Do some groups of youth bear a higher cost of joblessness than others? To address these questions, we selected a sample of all NLS youth age 14-21 who were not in the military, who had just left school in 1979, and who had wage and salary employment in 1984. A sample of 145 men and 164 women met these requirements and had full information on work experience between 1979 and 1984. For this sample, we estimated simplified earnings equations for men and women separately, including in our model the most significant variables from our previous analysis. Results are shown in Table 4.6.

Unemployment in 1979 did not have a significant impact on 1984 wages for men or women. For both groups, however, the number of weeks worked had a strongly significant impact. For men, each additional week of work since leaving school was related to an increase in wage and salary income of two-tenths of one percent (Table 4.6). For women, the increase in wage and salary income was three-tenths of one percent. This sample of youth averaged a little over three years of work experience in 1984, compared with an average of five and one-half to six years for our larger sample of all youth who were out of school in 1979. In the larger sample of youth, each week of work experience was associated with only a one-tenth of one percent increase in 1984 earnings for all but white women (Table 4.2, columns 1, 3 and 4). The

Table 4.6 Regression Analysis of the Effects of Early Unemploment on 1984 Hourly Earnings, Youth Leaving School in 1978, by Sex

Dependent Variable: Natural Log of Hourly Wage	Males	t-value	Females	t-value
Independent Variables:				
Constant	5.467	24.44	5.355	14.91
Black	-.113	-1.63	.190	1.90
Hispanic	.146	1.85	.072	.66
Union	.323	4.56	.110	.87
Weeks of Work Since Leaving School	.0021	4.11	.0032	4.63
Weeks of Tenure	.004	2.44	.003	1.17
Weeks of Training	.0002	1.93	-.000	-1.12
Years of Education	.027	1.21	.002	.05
High School Degree	.034	.37	.012	.08
Weeks of Unemployment, 1979	-.001	-.48	-.007	-1.25
Sample Size	145		164	
Adjusted R^2	.415		.193	
F	12.330		5.340	

Universe: Out-of-school youth who were not in the military, who left school in 1978 and who had wage and salary employment in 1984.

1984 earnings of white women were raised by two-tenths of one percent for each week of work experience (Table 4.2, column 2). Thus, joblessness in the early years after leaving school involves a higher cost than the average cost of joblessness for a sample of youth with much longer work careers.[4] A long period of joblessness in the first 4 years after leaving school imposes some costs on subsequent earnings, but the costs are not extremely high. Six months of joblessness reduces 1984 earnings by 5 percent for young male school leavers (24 x 0.21 percent) and by 8 percent (24 x 0.33 percent) for young females.

To determine if the years immediately after leaving school were more critical than others was difficult due to the collinearity of weeks worked from year to year for youth. Nevertheless, Table 4.7 shows the results of separating weeks of work since leaving school into each year's component. The terms were added stepwise into the regression equation (results not shown here), producing a pattern of results for men which was very similar to those shown in Table 4.7. Consistently the two years 1980 and 1982 had important effects on 1984 earnings (they are the only years which are statistically significant in Table 4.7), but the first year out of school was not important. It is unclear why 1980 and 1982 should be significant when other years were not. U.S. unemployment rates were extremely high in 1982, but they were equally high in 1983 and were considerably lower in 1980. The high degree of intercollinearity among the variables from year to year may be the explanation, making it difficult to conclude that work experience immediately after leaving school was unimportant for young men.[5]

For women, most of the separate years of work experience showed weak statistical significance when entered into the earnings equation, with the important exception of weeks worked in the first year after leaving school.

Table 4.7 Regression Results by Sex of the Effects of Each Year's Work Work Experience on 1984 Hourly Earnings, Youth Leaving School in 1978

Dependent Variable: Natural Log of Hourly Wage	Males	t-value	Females	t-value
Independent Variables:				
Constant	5.561	24.41	5.375	14.70
Black	-.129	-1.87	.207	2.01
Hispanic	.130	1.67	.077	.68
Union	.323	4.51	.114	.86
Weeks of Tenure	.006	2.96	.003	1.04
Weeks of Training	.0002	1.80	-.0003	-.95
Years of Education	.023	1.00	-.005	-.14
High School Degree	.072	.80	.045	.30
Weeks of Work, 1983	-.0012	-.52	.0036	1.11
Weeks of Work, 1982	.0032	1.73	.0026	.88
Weeks of Work, 1981	-.0001	-.05	.0034	1.09
Weeks of Work, 1980	.0079	3.82	.0021	.82
Weeks of Work, 1979	-.0025	-1.13	.0050	1.93
Sample Size	145		164	
Adjusted R^2	.441		.172	
F	10.45		3.82	

Despite the high degree of intercorrelation with other years of work experience, weeks worked in 1979 remained a statistically significant determinant of earnings 5 years later (Table 4.7). For women, a high degree of participation in the labor force immediately following school may proxy for a strong commitment to the labor force which, in turn, becomes reflected in high earnings in 1984. Alternatively, immediate post-school work experience may be important in its own right. Whichever interpretation is correct, the effect of work experience in 1979 was stronger than virtually any other variable in the young women's earnings equation.[6]

III. SUMMARY

Should the U.S. government fund special youth employment programs because of the long-run impact of teen-age unemployment? Data from the late 1970s and early 1980s, when youth unemployment rates were high, show that intervention in youth labor markets cannot be defended on this basis alone. Early unemployment did not have a significant negative impact on long-run wage rates for any group other than minority women. Even for female minority youth, our analysis showed that early unemployment was positively linked with long-run earnings for those who did not experience unemployment of long duration. Only when the duration of unemployment exceeded 24 weeks was early unemployment linked with reductions in long-run earnings.

For white male youth, the relationship between early unemployment and long-run earnings was positive--as early unemployment increased, so did long-run earnings. Contrary to our expectations, the positive relationship was not linked to productive search-unemployment. Young men who changed jobs while unemployed earned no more than young men whose unemployment did not accompany a change of employers. Our data did not prove it, but men with high-unemployment jobs may have been compensated by receiving higher long-run earnings.

Though unemployment itself was not linked with reduced subsequent earnings, joblessness, particularly in the period immediately after leaving school, had a significant impact on earnings. For our sample of youth who were out of school for approximately 5 years, the cost of each week of joblessness was about the same, regardless of race and sex. White males, minority males, and minority females lost about one-tenth of one percent of 1984 wages for each week of joblessness experienced after age 18. White females lost about two-tenths of one percent of 1984 wages for each week of joblessness.

Joblessness in the period immediately following school was more costly than it was later in a youth's working career. We estimated that each week without work in the first four years after leaving school was associated with two-tenths of a percent loss in wages for men and three-tenths of a percent for women. Thus for women, a long spell of joblessness early in time would be more costly than for men. Six months of joblessness would have reduced 1984 wages by 5 percent for men and 8 percent for women. These effects are not large compared to the 22 to 31 percent wage increment which accrued to youth who ultimately found unionized employment (Table 4.2). Women, nevertheless, experienced more total joblessness than men in the four years after leaving school. Even the women in our sample who were employed at some time in 1984 worked less than men during this post-school period, with weeks employed averaging 165 weeks for men and 149 weeks for women. Thus the total cost of joblessness was considerably larger for women than for men.

In summary, youth unemployment has few significant depressing effects on long-run earnings over and above the loss in work experience which accompanies unemployment. Government policy makers should focus on the costs of youth joblessness and not on the effects of early unemployment on youth attitudes or on the hiring practices of potential employers.

Chapter 4

Notes

[1] Work experience is often measured very crudely as current age minus the age when last left school, for instance. Note that our definition of work experience is simply the number of weeks worked since a given date (e.g., date of leaving school or date at which respondent was age 18). The term "work experience" does not refer to special government programs which provide specific kinds of work experience.

[2] The approximation holds for small values for the regression coefficient. Large coefficients are converted to percentage changes using the following formula: e^x-1 where x = the regression coefficient.

[3] The coefficients for "union" in regressions by race and sex are converted to these percentages using the formula described in footnote 2 above.

[4] To make sure the difference in estimates was not due to different regression models, we reestimated the model for the larger sample of youth with the same reduced set of variables used in the smaller sample of school leavers. The work experience variable retained a value of approximately 0.1 percent for men and women alike.

[5] The correlation coefficient between weeks of work in 1979 and weeks of work for young men in 1980 was .641, for example.

[6] Note, however, that the predictive power of all the variables in the young women's earnings equation was low.

Chapter 4

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Chapter 5

Within-Family Variation in the Educational and Early Occupational Attainment Process*

by

William R. Morgan

The educational attainment of the average American youth is subject to a variety of influences. Some of the most critical decisions are made within the family, by the parents and siblings of the youth. When scarce family resources, both economic and psychic, are invested in the children's education, are there systematic tendencies for some children within the family to benefit more than others? If some do benefit more, is the outcome distribution consistent with need-based, merit-based, or privilege-based allocation rules? Or are parents blindly egalitarian? As yet there is little systematic general knowledge of these within-family influences. This research will examine the extent of variation in the educational progression of youth from the same family, and then explain that variation by estimating a sibling difference model of educational attainment.

Although several theoretical perspectives are revelant to this problem, the primary one is the status attainment or stratification model of sociology exemplified by the Wisconsin panel model developed over the past 20 years by Sewell, Hauser, and associates (Hauser et al., 1983). They have encapsulated the complexities of status attainment into a parsimonious causal model in which educational and occupational attainments are determined by the person's socioeconomic origins and mental ability, as these are mediated by high school achievement, influence from significant others (parents, peers, teachers) and subsequent aspirations for attainment. One gap in the model is that it permits no assessment of possible within-family variations. Sewell and Hauser (1983) have sought to remedy this deficiency by extending their data set to include detailed information on siblings. Their initial analysis examined the effects of birth order and sibship size on attainments. They found the commonly-reported negative effect of sibship size, but contrary to much previous work (e.g., Zajonc and Markus, 1975; Mott and Haurin, 1982), no birth order effect. They attribute the discrepancy to greater methodological rigor in their study. The initial conclusion thus is that any refinement of their model to include explanations of within-family variation is unnecessary. Their analysis, however, barely skimmed the surface of theoretical expectations for within-family variation.

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A second theoretical perspective is offered by the recent work on the micro-economics of the family. Based originally on the human capital model of Becker (1975), Becker and Tomes (1976) predicted parents would seek to maximize the expected earnings of their offspring and so would invest more resources in their most able children. This finding contradicted Olneck's (1977) careful empirical analysis which showed a similarity in the earnings of brothers that exceeded what could be attributed to equivalence in ability. Olneck's paper, plus two more theoretical efforts (Behrman, Pollak, and Taubman, 1982; Sheshinski and Weiss, 1982) originating in the "kinometrics" conference organized by Taubman (1977), abandoned the investment model of human capital theory for a consumer choice, utility maximization approach. These models incorporate the assumption that parents have an aversion to inequality and will bequest in a compensating rather than reinforcing mode to offset the higher expected earnings of the more able sibling. Limited support is obtained using twin data, although their latent variable approach requires numerous measurement assumptions. One disconfirmation had to do with Sheshinski and Weiss's formal derivation that within-family variations in expected earnings would exceed between-family variations. All empirical evidence to date runs contrary to this expectation.

A third useful perspective is the social psychology of small group interaction. In particular, the expectation states theory of the Stanford school (Berger et al., 1972) specifies a general tendency for task groups to allocate scarce resources and rewards on the basis of generalized status characteristics such as gender and age, particularly if not countermanded by specific task performance information. Whether or not the American family operates in this manner has not been determined, although one would expect a primary socialization group to differ from a task group. The work of Michener and Lawler (1971) suggests power relations within small groups, including families, may be at least as important as these normative considerations. Some parents may allocate resources to maintain parental control and authority and minimize "revolutionary coalitions" among their offspring (Caplow, 1968). The presence or absence of sibling rivalry (Felson, 1983; Morgan, 1983a), may also affect allocation decisions.

The sibling difference model of educational attainment that will be tested in this paper draws upon each of these perspectives. The basic structure of the status attainment model is retained: family background factors, mediated by schooling experience and significant others' influence, determine subsequent educational achievement and attitudes. There are, however, two differences of some importance. The first is that the sibling difference equation estimation procedure to be used effectively subtracts out any variables from the equation that are common to siblings of the same family. These are the family background variables, including those that are measured as well as those that are not. In addition to increased parsimony, this procedure removes from the estimation equations the potential bias from having unmeasured family variables in the disturbance term.

A second variation on the basic model is the representation and interpretation of the standardized mental test score measure, which in the NLS youth data is represented by the AFQT (Morgan, 1983b). The conventional model would represent this measure as an ability variable exogenous with respect to schooling. As many have noted, however, such measures taken during or after a youth's high school career contain both prior ability and school achievement components. Differencing AFQT scores permits removal from the equation that component of each sibling's ability that is shared among offspring. Each sibling's AFQT difference score thus represents his/her relative amount of uniquely-inherited ability and uniquely-determined school achievement.

The micro-economics perspective is the basis for representing the mediating variable of parental socialization as a rational resource allocation process. The pattern of relationships between, on the one hand, sibling-reported differences in amounts of parental encouragement and financial support for college and, on the other, status characteristic differences between siblings, will offer evidence as to which allocation rules predominate. Encouragement and support for the more able, better performing offspring would support the Becker and Tomes meritocratic, return-on-investment allocation rule. Conversely, greater support for the less able and/or lower status offspring would suggest compensatory allocation, consistent with Sheshinski and Weiss's parental aversion to inequality principle. The other possible empirical pattern, in which amounts of parental encouragement and support are random with respect to ability and status characteristics, would be consistent with blind egalitarianism.

The third perspective, expectation-states theory, suggests that the most important generalized status characteristics in families and indeed in most all primary groups are relative age or seniority and gender. All other things equal, society traditionally places a higher value on older males, and similarly within the family senior brothers tend to be accorded higher status. The potentially overriding performance criterion, relative high school academic achievement, should be particularly relevant with respect to the encouragement and financing of college. As emphasized above, however, the relative importance of these status and performance criteria within the family and even the direction in which parents respond to them is still very much an empirical question.

One potentially serious flaw in this empirical examination of parental allocation behavior is the theoretical assumption of an essentially static parental decision-making apparatus. Like all units of social organization each family has a history; thus the most valid representation of parental decision-making would incorporate its dynamic properties. For example, optimally one should incorporate possible differences in the available family resources during the initial college years of each sibling. Furthermore, parents may learn from their mistakes and successes with their earlier-born offspring and adjust their allocations to their later-borns accordingly. In this paper we shall assume these omitted dynamic effects operate randomly with respect to the sibling characteristic variables included in the present model, the possible exception being relative age. To the extent that unmeasured dynamic properties of families cause a sequential and linear pattern of decision adjustments, caution should be exercised in the interpretation of this variable.

I. SAMPLE, VARIABLES, AND ESTIMATION PROCEDURE

The household sampling plan of the youth cohort generated 5,863 respondents who had between one and 5 siblings also included in the survey. Of these youth, 57.8 percent had one other sibling who was sampled, 29.4 percent had 2, and 12.8 percent had between 3 and 5 siblings. Previous analysis (Morgan, 1983a) indicated this sibling sample was comparable to the full youth sample in its representativeness of the corresponding population. In this report two sibling subsamples of 1984 (Round 6) respondents are drawn. First is the high school sibling sample, consisting of 1,943 youth who were of high school age (14-17) in 1979 and had at least one other sampled sibling also in that age range. Second is the college sample, consisting of the 1,195 youth who as of 1984 had enrolled in a 2 or 4-year college by age 19 and had at least one sibling enrolled. The first sample will be used to examine sibling differences in parental encouragement of college, and the second examines differences in family financial assistance for college. Table 5.1 gives the descriptive statistics for the 9 family background variables included in the analysis of each sample. As expected, the high school siblings have a broader socioeconomic and ethnic distribution than the college siblings.

Table 5.2 presents similar descriptive statistics for the sibling variables used in both the high school and college level analyses. These statistics are given for the total (X_{ij}) and difference ($X_{ij}-\bar{X}_j$) form of each variable, where the latter is defined by subtracting each respondent's total value (X_{ij}) from the corresponding mean value ($\bar{X}_j$) for all sampled siblings from each respondent's family unit. The fact that the means of these sibling difference scores tend to diverge slightly from their expected zero values is

Table 5.1 Descriptive Statistics of Exogenous Family Background Variables in the High School and College Sibling Samples

Background Variables	High School (n=1,943) Mean	S.D.	College (n=1,195) Mean	S.D.
Ethnicity (white)[a]				
Black	.160	.367	.089	.285
Hispanic	.059	.236	.033	.178
Region (Pacific)				
Northeast	.189	.392	.262	.440
South	.299	.458	.272	.445
North Central and Mountain	.401	.490	.358	.480
Urban (rural)	.782	.413	.819	.385
Family income (0-10)				
10-20,000	.266	.442	.152	.359
20-30,000	.233	.423	.237	.426
30,000 +	.173	.379	.333	.472
NA	.163	.370	.197	.398
Father's education (0-11)				
12	.315	.465	.250	.433
13-15	.111	.314	.157	.364
16	.114	.318	.231	.422
17-20	.076	.266	.190	.393
NA	.096	.295	.040	.196
Mother's education (0-11)				
12	.424	.494	.444	.497
13-15	.125	.331	.177	.382
16	.080	.272	.205	.404
17-20	.025	.157	.058	.234
NA	.048	.214	.024	.152
Father's occupation (No Emp.)				
White collar	.288	.453	.559	.497
Blue collar	.457	.498	.279	.449
Mother's occupation (No Emp.)				
White collar	.262	.440	.354	.478
Blue collar	.273	.446	.141	.348
Siblings, no.	3.83	2.30	3.20	1.99

[a] Reference category of dummy variables in parentheses.

Table 5.2 Descriptive Statistics of Exogenous and Endogenous Sibling Variables, in Total Individual (X_{ij}) and Within-Family Deviation ($X_{ij}-X_j$) Form

Sibling Variables	Total Form Mean	Total Form S.D.	Within-Family Deviation Form Mean	Within-Family Deviation Form S.D.
A. High School Sample (n=1,949)				
Gender, male	.512	.500	.006	.355
Age, 1984	21.0	1.15	-.436	1.16
AFQT	67.3	20.6	-.676	9.93
Parental encouragement	2.96	.894	.000	.579
College prep. curriculum	.331	.471	-.001	.308
Private high school	.057	.233	.002	.102
Expected education, years	13.9	2.20	-.038	1.29
Expected occupation, Duncan SEI[a]	52.8	24.2	-.407	16.0
Complete high school by age 19	.794	.404	-.005	.246
Attend college by age 19	.450	.498	.002	.293
B. College Sample (n=1,195)				
Gender, male	.490	.500	.002	.374
Age, 1984	22.8	2.13	.033	1.59
AFQT	84.4	14.4	.778	8.12
College prep. curriculum	.602	.490	.021	.333
Private high school	.121	.326	.005	.127
Average annual loan dollars	522	994	17.0	625
Average annual grant dollars	529	1,240	5.2	792
Four-year first college	.689	.463	.001	.261
Family financial contribution	1.47	1.17	.025	.694
Self financial contribution	1.23	1.07	-.015	.674
College years attended, 1984	3.11	1.48	.019	.963

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[a] Sample size for analyses using this variable is 1,640.

due to computational rounding errors and the use of all available siblings in generating family means, even if they did not meet the respective age and attendance criteria for inclusion in the two samples.

The outcome variables to be explained in the high school analysis include sibling differences in their 1979 estimates of expected educational attainment, measured in years, and expected occupational attainment at age 35, measured in Duncan Socioeconomic Index units. Also examined are sibling differences in actual educational attainment, including completion of high school by age 19 and college attendance by age 19. In the college-level analysis the attainment difference measure is college progression level, measured as the highest year of college attended by 1984.

Both analyses employ as exogenous variables sibling differences in the status and performance characteristics of gender, age, and AFQT. Both analyses also include two control variables for differences in quantity of high school academic coursework, these being enrollment in a college preparatory curriculum and attendance at a private high school. The college analysis employs additional controls for sibling differences in college costs and external financing arrangements. These three variables, again in difference form, are whether the first college attended was a 4-year versus a 2-year institution, the average annual loan dollars used for each year at this institution, and similarly the average annual grant dollars received.

The parental encouragement of college variable was the 1979 report of how strongly, on a 4-point scale, one or both parents would disapprove if the respondent decided not to attend college. Strongly disapprove signified high parental encouragement. When transformed to sibling difference form this variable represents parent's relative encouragement allocation to each sibling. Family financial contribution to college was the 1984 report of how much each respondent's family had paid or was paying of all the respondent's college expenses not covered by loans or grants. This measure was scaled in four steps ranging from none of the expenses to less than half, half or more, or all. In difference form the variable is each sibling's relative amount of family financial assistance, representing parent's relative economic allocations to each sibling. One other variable in the college sample analysis was a control for the respondent's own financial contribution, from earnings and savings, measured on an identical 4-step scale.

Table 5.3 demonstrates how the sibling difference estimation procedure, performed on the college attendance equation for the high school sample, differs from two related estimation approaches. Column one reports a standard ordinary least squares equation where each sibling's total score on the outcome variable is regressed on the total scores for the full set of 9 family background and 6 sibling characteristic variables. To facilitate comparison, only the estimated coefficients for the sibling variables are displayed. In this first procedure, the significant parental encouragement effect on college attendance represents for each youth the average combined effects due to differences both within and between families on this variable. It permits no separate assessment of within-family parental allocation behavior. Furthermore, these estimates of the effects of parental encouragement and sibling characteristics are biased to the extent that any of these variables are correlated with critical family background variables that were omitted from the equation.

The equation in the second column nets out the effects of between-family variation in levels of parental encouragement and the other 5 sibling characteristics. This calculation was made by representing the resppondent's total score (X_{ij}) for each of these variables by its two orthogonal components

Table 5.3 OLS Regression Coefficients for Sibling Characteristic Effects on Youths' College Attendance by Age 19, Estimated Separately for Three Variable Forms (n=1,943)

Sibling Characteristic	Total Individual Variation[a] (X_{ij}, Y_{ij})	Within Family Variation[b] ($X_{ij}-\bar{X}_j$, Y_{ij})	Sibling Difference Variation ($X_{ij}-\bar{X}_j$, $Y_{ij}-\bar{Y}_j$)
Gender, male	-.030 (-1.72)[c]	-.044 (-1.81)	-.041 (-2.41)*
Age	-.008 (-1.06)	-.017 (-2.02)*	-.022 (-4.05)*
AFQT (x10)	.087 (14.97)*	.097 (10.56)*	.100 (14.98)*
Parental encouragement	.080 (7.83)*	.044 (2.98)*	.041 (3.91)*
College prep. curriculum	.185 (8.79)*	.144 (5.00)*	.167 (8.26)*
Private high school	.008 (.20)	.032 (.38)	.051 (.87)
R^2	.435	.441	.182

*$p < .05$

[a] Estimated net of 9 family background variables.

[b] Estimated net of 9 family background variables and family mean values of each sibling characteristic.

[c] t-ratios in parentheses.

--the sibling mean score ($\bar{X}_j$) for each family average from all sampled siblings in that family, and each sibling's deviation ($X_{ij}-\bar{X}_j$) from that mean score. This equation is otherwise similar to the first in that it includes the 9 family background variables and estimates the total score (Y_{ij}) representation of the endogenous college attendance variable. A comparison of the R^2 values for these first two equations indicates at best a slight improvement in fit from using the elaborated between and within-family components model. One could in fact argue that the more parsimonious first equation is preferable, on the grounds that the two effects due to the between and within-family differences on each sibling variable are sufficiently similar to be adequately measured by the average of the two.

The third column reports the sibling difference estimation procedure used in this report, where all exogenous and endogenous variables are represented in the equation in difference score form ($X_{ij}-\bar{X}_j$, $Y_{ij}-\bar{Y}_j$). Its principal advantage over the second procedure is that it removes through differencing any common unmeasured family background variables from the disturbance term that would otherwise bias the estimated effects of the sibling characteristics (Rosenzweig and Wolpin, 1984). The equation is also more parsimonious, since differencing also results in deletion of the 9 family background variables common to each sibling of the same family. Most importantly, this sibling difference operationalization best represents the theoretical meaning assigned to the variables.

II. FINDINGS

A. Parental Allocation Behavior

The overall pattern of results indicates that American parents do indeed allocate differentially depending on the status and performance characteristics of their offspring. During the high school years, parents' level of encouragement responds positively to performance and negatively to status (Table 5.4). At the college level, their economic allocations respond negatively to status characteristics, but are no longer significantly influenced by their offsprings' prior high school performance (Table 5.5). It is only their psychic allocations during the high school years, however, that have much impact on their offsprings' differential educational attainments. These findings broadly suggest a picture of the average American parent being jointly driven by meritocratic and compensatory propensities in their resource allocations to offspring. In other words, their allocation criteria appear to reflect both a human capital, investment-oriented decision process and a need-responsive aversion to inequality preference function.

Table 5.4 reports the results of the high school sample analysis. Significantly more parental encouragement went to those siblings who were better academic performers, but also to those with the sibling status disadvantages of female gender and younger age. The greater encouragement given to those siblings generated a significant relationship for them for all four of the expected and actual attainment variables. This effect operated more strongly on the two expectations measures, however.

In addition to their significant indirect effects via parental encouragement, these three sibling characteristics had additional direct effects on the four attainment variables. A higher AFQT score produced a relative advantage in expected education and occupational attainments, and in the likelihood of completing high school and attending college. The strongest direct effect was for college attendance. Being one standard deviation (10 points) higher than the family average on the AFQT put a youth 10 percentage points above the sibling average for his family in the likelihood of attending

Table 5.4 OLS Regression Coefficients for the Sibling Difference Model of the High School Achievement Process (n=1,943)

Explanatory Variables[a]	Parental Encouragement	Expected Educational Attainment	Expected Occupational Attainment[b]	Completed H.S. by Age 19	Attended College by Age 19
Gender, male	-.110* (-2.99)[c]	-.041 (-.55)	-4.67* (-4.50)	-.035* (-2.29)	-.041* (-2.41)
Age	-.030* (-2.61)	-.011 (-.46)	-0.34 (-1.06)	-.005 (-.99)	-.022* (-4.05)
AFQT (x10)	.054* (4.00)	.377* (13.38)	4.08* (10.42)	.051* (8.77)	.100 (14.98)*
Parental encouragement	--	.444* (9.62)	3.42* (5.21)	.021* (2.17)	.041 (3.91)*
College prep. curriculum	--	.693* (7.83)	4.85* (3.90)	.020 (1.11)	.167 (8.26)*
Private high school	--	-.186 (-.72)	5.62 (1.49)	.278* (5.22)	.051 (.87)
Constant	-.010	-.015	-0.28	-.003	-.000
R^2	.015	.189	.122	.067	.182
$R^2_{P,B}$	.102	.417	.213	.210	.383
$R^2_{P,B,W}$[d]	.110	.475	.264	.220	.442
$1-R^2_{P,B}/R^2_{P,B,W}$	.072	.122	.193	.045	.133

* $p < .05$

[a] The values for all exogenous and endogenous variables in the sibling difference model are each respondent's deviation score from his/her respective family mean score on that variable.

[b] Sample size for expected occupational attainment is 1,640.

[c] Sample size for expected occupational attainment is 1,640.

[d] P denotes parental background heterogeneity, B notes between-family heterogeneity in sibling characteristics, and W denotes within-family heterogeneity in sibling characteristics. See text for details.

Table 5.5 OLS Regression Coefficients for the Sibling Difference Model of College Progression (n=1,195)

Explanatory Variables[a]	Average Annual Loan	Average Annual Grant	First Coll. 4-Year	Family Finan. Contrib.	Self Finan. Contrib.	Highest Yr. Coll. Attended by 1984
Gender, male	19.2 (0.40)[b]	104.7 (1.73)	-.034 (-1.76)	-.169* (-3.22)	.154* (3.03)	.094 (1.51)
Age	5.1 (0.42)	-22.5 (-1.46)	-.011* (-2.17)	-.038* (-2.86)	.057* (4.45)	.234* (14.70)
AFQT (x10)	7.8 (0.75)	145.9* (4.77)	.074* (7.48)	-.051 (-1.86)	.113* (4.28)	.280* (8.63)
College prep. curriculum	-41.4 (-0.75)	-46.1 (-0.67)	.070* (3.12)	.025 (0.41)	-.042 (-0.72)	.090 (1.27)
Private high school	289.5* (2.00)	502.2* (2.77)	-0.081 (-1.38)	-.061 (-0.38)	.066 (0.44)	.219 (1.18)
Average loan ($x10^3$)	--	--	--	.006 (0.18)	-.061* (-2.00)	.049 (1.31)
Average grant ($x10^3$)	--	--	--	-.143* (-5.71)	-.059* (-2.45)	.070* (2.29)
Four-year first college	--	--	--	.245* (3.14)	-.146 (-1.93)	.447* (4.94)
Family contribution	--	--	--	--	--	.047 (1.25)
Self contribution	--	--	--	--	--	-.017 (-0.43)
Constant	15.5	-7.3	-.006	.030	-.026	-.015
R^2	.004	.029	.061	.058	.063	.320
$R^2_{P,B}$	.117	.073	.202	.212	.105	.290
$R^2_{P,B,W}$	.120	.085	.221	.231	.130	.425
$1-R^2_{P,B}/R^2_{P,B,W}$[c]	.026	.141	.086	.082	.192	.318

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* $p < .05$

[a] The values for all exogenous and endogenous variables in the sibling difference model are each respondent's deviation score from his/her respective family mean score on that variable.

[b] t-ratios in parentheses.

[c] P denotes parental background heterogeneity, B notes between-family heterogeneity in sibling characteristics, and W denotes within-family heterogeneity in sibling characteristics. See text for details.

college. Less expected was the finding that being female and younger also resulted in significant advantages relative to male and older siblings in attendance likelihood. Only the relative AFQT performance measure affected all four measures, however, with female gender having significant effects on three of the measures, and relative age only on college attendance likelihood. These unexpected direct effects of status characteristics call for further investigation. One possible explanation is that initially disadvantaged siblings engage directly in their own compensatory behavior, and in essence "try harder." The currently widespread endorsement of feminist cultural values may help to encourage such effects by sisters.

Table 5.5 reports the analysis of the college sample. As with parental encouragement, the amount of family financial assistance is larger for college-attending siblings who are female and younger. Conversely, those siblings contribute a relatively smaller share from their own earnings and savings. But unlike parental encouragement allocations, the relative amount of family financial assistance was no longer related to prior high school performance as measured by relative AFQT score. Furthermore, sibling status characteristics had no effect on relative amounts of external financial assistance from loans and grants. The performance variable, by contrast, did raise relative grant size.

The measure of attainment used for the college sample, differences in highest year of college attended by 1984, showed few significant relationships after controlling for sibling age differences. Other than for age, the only strong effects of the 10 variables in this equation came from relative AFQT score and beginning at a four-year institution. Differences in average annual grant dollars did increase attainment differences, but differences in loan dollars did not. Importantly, relative amounts of both family and own financial contributions had no effect on differences in college attainment. Thus, even though differences in family economic allocations were patterned in the same compensatory direction as parental encouragement, these differences had little bearing on which sibling attained more years of college. It may very well be that at this stage of development youth are sufficiently autonomous from the family context to make parental differences in allocation behavior less critical to them. The significant effect of grant size differences on relative college attainments further suggests that the availability of external financial assistance is another important factor in loosening this family dependence.

B. Secondary Schooling Differences

Sibling differences in high school academic opportunities represent the other main social factors besides family socialization differences that account for differential attainments of siblings. Over the entire sibling sample approximately one-half the youth in a college preparatory curriculum had one or more sampled siblings who were in a different curriculum. Similarly, one-third of those in a private school had one or more siblings in a public school. These differences were sufficient to make it important to include these two variables in the sibling difference equations in order to obtain an unbiased estimate of the parental allocation effects. These variables have interest on their own, however, particularly in view of the continuing controversy over the relative merits of public and private schools as learning environments (e.g., Coleman, Hoffer and Kilgore, 1982; Morgan, 1983b; Jencks, 1985).

A focal point of controversy in this research has been how best to remove the student selectivity bias from the sector and curriculum effect estimates. Since the sibling difference procedure removes any bias from the equation due to socioeconomic and other family background differences, superficially at least one could argue that the present design offers a superior method for controlling this selection. In reality, however, the examination of sector and curriculum variation within families raises a whole new set of selectivity issues dealing with the individual differences between siblings who have been placed in one or the other sector and curriculum.

To a large extent sibling schooling opportunity differences result from parental decision processes. The previous finding of parents' dual propensity toward meritocratic and compensatory treatment of their offspring is therefore instructive. What it suggests, however, is simply a low likelihood that any single-direction hypothesis can adequately explain the factors underlying why parents sometimes guide their offspring into different sectors and curricula. Consequently it will be necessary to side-step this within-family selectivity issue until a time when better information on parental choice processes becomes available. These schooling findings will thus be presented with the prior understanding that they are only a first approximation of causal effect estimates, containing as they do an unknown element of sibling selectivity bias. In the present analysis procedure the absence of bias could only be assured if the sibling placement differences in curricula and sectors were random events, and such an assumption is dubious.

The curriculum and sector effects were estimated in the same sibling difference equations that generated the parental allocation estimates (Tables 5.4 and 5.5). In this context, however, the other 4 variables in the equation assume a revised theoretical meaning. The parental encouragement variable controls for family socialization differences, but in itself is not a central focus. Gender and relative age, while still denoting key status characteristics within the family, in the school context are better understood as indicators of sex-role behavior and mental-maturation differences. The meaning of AFQT as a high school academic performance indicator remains the same.

Looking first at the high school sample, sector and curriculum differences between siblings did significantly increase attainment differences, but with the direct effects of curriculum tending to be larger and more widespread than those for sector. The sibling curriculum differences significantly heightened differences in educational and occupational expectations and college attendance, to the advantage of the sibling in a college preparatory curriculum. Sector differences directly increased differences only in the likelihood of high school completion, to the advantage of the private school sibling. For the college sample, neither schooling difference affected differences in highest year of college attended, although there were two other suggestive relationships. The sibling in a private school received significantly higher college financial assistance, both from loans and grants, and the college preparatory sibling was more likely to first attend a four-year instead of a two-year institution.

Two additional analyses estimated the effects of these sibling school differences on 10 achievement scores and 6 quality of student life dimensions (Tables 5.6 and 5.7). These two sets of variables were the academic and vocational subtests of the Armed Services standardized test battery, administered in 1980 to the entire cohort, and the school attitude items collected from all current high school enrollees in the first round 1979 interviews. The first 4 academic subtests when combined constitute the AFQT summary measure of overall high school performance.

Table 5.6 OLS Regression Coefficients for the Sibling Difference Model of Youths' 1980 Academic and Vocational Achievement Scores, for the High School Sample (n=1,943)

A. Academic Explanatory Variables	AFQT Composite	Paragraph Composition	Word Knowledge	Arithmetic Reasoning	Numerical Operations	Mathematics Knowledge	Science Knowledge
Gender, male	-1.874* (-3.12)	-.799* (-6.58)	-.287 (-1.24)	1.060* (4.59)	-3.65* (-9.82)	-0.011 (-0.05)	1.372* (8.77)
Age	-1.887* (10.30)	.269* (7.26)	.749* (10.61)	.548* (7.77)	.638* (5.61)	.383* (6.29)	.636* (13.30)
Parental encouragement	1.096* (2.96)	.146 (1.94)	.124 (.87)	.552* (3.87)	.544* (2.36)	.418* (3.40)	.157 (1.63)
College prep. Curriculum	7.226* (10.43)	1.348* (9.60)	2.425* (9.09)	2.465* (9.25)	1.959* (4.56)	2.75* (11.95)	1.680* (9.30)
Private high school	4.611* (2.20)	.596 (1.40)	1.858* (2.30)	2.981* (3.70)	-1.775 (-1.36)	1.504* (2.16)	1.413* (2.58)
Constant	.169	.023	.041	.036	.138	.114	.057
R^2	.109	.092	.092	.090	.077	.094	.148

B. Vocational Explanatory Variables	Mechanical Comprehension	Auto & Shop Knowledge	Electronics Information	Coding Speed
Gender, male	3.195* (19.68)	4.214* (26.25)	2.442* (17.88)	-8.222* (-14.24)
Age	.387* (7.80)	.604* (12.32)	.597* (14.31)	1.127* (6.39)
Parental encouragement	.129 (1.28)	.084 (.85)	.200* (2.37)	.076 (.21)
College prep. curriculum	1.152* (6.14)	.682* (3.68)	1.047* (6.64)	5.042* (7.56)
Private high school	1.242* (2.19)	.580 (1.03)	1.856* (3.89)	2.034 (1.01)
Constant	.032	.067	.085	-.063
R^2	.200	.305	.229	.136

* $p < .05$

Table 5.7 OLS Regression Coefficients for the Sibling Difference Model of Youths' 1979 Ratings on Quality of Student Life, for All Siblings Attending High School in 1979 (n=1,677)

Explanatory Variables	Instructional Quality	School Discipline	Learning Freedom
Gender, male	-.082 (-3.24)*	-.025 (-.67)	-.074 (-1.97)*
Age	-.000 (-.01)	-.060 (-5.12)*	.023 (1.93)
AFQT (x10)	.006 (.59)	-.001 (-.07)	.020 (1.41)
Parental encouragement	.060 (3.74)*	.067 (2.89)*	.013 (.53)
College prep. curriculum	.104 (3.49)*	.002 (.04)	-.006 (-.14)
Private high school	.324 (3.64)*	.012 (.09)	-.145 (-1.10)
Constant	-.003	-.020	.007
R^2	.034	.022	.008

	Personal Safety	Job Counseling	Peer Relations
Gender, male	.139 (3.90)*	-.066 (-1.71)	.069 (2.35)*
Age	-.007 (-.64)	.020 (1.68)	-.016 (1.71)
AFQT (x10)	.050 (3.72)*	.028 (1.92)	.003 (.30)
Parental encouragement	.018 (.82)	-.041 (-1.69)	-.030 (-1.60)
College prep. curriculum	.009 (.21)	.065 (1.45)	.012 (.35)
Private high school	.037 (.29)	-.267 (-1.98)*	.307 (2.97)*
Constant	.001	.012	-.010
R^2	.017	.013	.012

* $p < .05$

College preparatory siblings scored significantly higher than their general and vocational curriculum siblings on all 10 academic and vocational achievement subtests. In addition, private school youth scored significantly higher than their public school siblings on 4 of the 6 academic subtests and 2 of the 4 vocational subtests. Although how much these effects derive from the schooling experience itself or from prior selection is as yet uncertain, clearly siblings from the college preparatory curriculum and the private school sector enjoy a strong learning advantage over their general/vocational and public school counterparts.

Sibling ratings of student life quality also varied by schooling experience, but this time sector mattered somewhat more than curriculum placement. The sibling in a private school gave a higher rating to both the quality of instruction and peer relations, and only job counseling was rated higher by siblings in public schools. Curriculum differences affected siblings' relative ratings only of instructional quality, with college preparatory siblings giving the higher ratings. Differences in rating of school discipline, learning freedom, and personal safety were unrelated to either sector or curriculum differences.

These sibling effect findings are consistent with earlier results based on an analysis of individual youth from the full cohort (Morgan, 1983b). These results would suggest that differences in school experiences between youth from the same family tend to produce differential outcomes in approximately the same direction and degree as do differences in schooling between youth from different families. One or two discrepancies in the overall pattern of results call for further investigation, however. Most importantly, the sector effects on achievement scores tend to be larger in the sibling difference analysis. Sector differences in student life quality ratings tend to be smaller. Further analysis will be needed to determine the extent to which these discrepant sibling difference estimates reflect reduced between-family selectivity bias or instead simply new bias from between-sibling selectivity.

C. The Societal Impact of Sibling Heterogeneity

Whether or not these sibling difference effects should be a potential concern either for national policy or more modestly, for current formulations of social stratification theory, depends in part on what proportion of the total variation in the educational attainments of youth they explain. The conventional wisdom, as represented in the Wisconsin stratification model, is that most attainment variation is due to family-based socioeconomic origin differences. In other words, family position in the socioeconomic structure of American society has more impact on eventual educational attainment level than sibling position in the structure of the American family.

Three rows of R^2 values are presented at the bottom of Tables 5.4 and 5.5. The first gives the total variation in the endogenous sibling difference variables explained by the difference variables listed above. The next two values are for the total score version of each difference equation (corresponding to the column 2 equation in Table 5.3), when family background and sibling means, and then orthogonal within-family sibling deviations are successively entered. A fourth row gives the proportion of the total explained variation in this equation due to the within-family sibling differences. As expected, the overwhelming proportion of explained variation is due to family background differences. Over the 11 equations reported in the two tables, the proportion of the total explained variation due to the sibling difference variables averages only 12 percent. More importantly, for the two allocation measures, parental encouragement and family financial contribution, sibling differences account for only 7.2 percent and 8.2 percent of their total explained variation, respectively. Clearly, for American youth where their parents stand in the social structure affects how their parents treat them much more than where their own status and performance position is relative to their siblings. Is this study and its findings then much ado about nothing? I think not.

III. CONCLUSION

To some degree the findings of this study are counter-intuitive, although for many perhaps it is not so surprising to find that parents are both meritocratic and compensatory in their resource allocation. The surprise is the implication of such behavior, in that little sister, especially if she is relatively hard-working, apparently receives an edge over big brother that boosts her subsequent educational attainments. Leaving aside any judgments as to personal fairness or even the long-term implications of such behavior for society, it is interesting to speculate on the moral lessons to children from such treatment.

Certainly both modes of resource allocation are essential in American society, and it may be that these family allocations represent the primordial experience of such treatment. The lessons learned in the family socialization setting tend to be carried forward into one's own family, workplace, and other primary adult social role settings. Learning through interaction with parents and siblings that resource allocations are both need- and merit-based is therefore potentially a strong lesson. Even though Americans are constantly beset with value pronouncements that we are at once a hard-working and a caring society, many adults find experiencing the conflicting reality of compensatory and merit rewards to be personally troublesome. It may very well be that the ability and readiness to accept the legitimacy of both principles, particularly when the application of one works to one's own short-term disadvantage, depends in no small part on a person's childhood experiences with these conflicting criteria.

The parental allocation behavior that was examined in this research took place at only one special stage in their children's life cycle, the period from middle to late adolescence. Even over this relatively short period, however, the findings suggested that the allocation criteria used by parents may change. Earlier psychic allocations tended to be both need- and merit-based, while later financial allocations tended to be more strictly need-based. If this research had examined the cohort at an even earlier point in the life cycle, parental allocation behavior might have been different again.

Furthermore, regardless of how changing parents may be over the children's life cycle in their allocation principles, youths' possession of the characteristics on which parents apply their principles is never constant. Most obviously, normal fluctuations in school performance make it likely that parental applications of the merit principle will serve at varying times to reward and to deprive a given child. Similarly, when parents define need on the basis of relative age, the inevitability of age maturation and the likelihood of sibling succession makes it likely that at different points in the family life cycle age will work both for and against a given child.

The use of gender as an allocation criterion is more troublesome, and parents' continued use of it as a status criterion probably ill equips both daughters and sons for contemporary gender-free definitions of adult role behavior.

Chapter 5

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Chapter 6

Why Does Work in High School Matter?

by

Ronald D'Amico

The last several decades have witnessed a more than doubling of the rate of employment among high school youth. This burgeoning rate of labor force participation was consistent with, though not necessarily caused by, the repeated directives of various national commissions (e.g., National Panel, 1974) that the employment of high school youth should be strongly encouraged. The reason for this mandate was the belief that high school employment would encourage responsible attitudes and behaviors, decrease dependency, and ease the transition from school to work.

Recent evidence does suggest that youth who work in high school show greater self-reliance (Greenberger, 1983), find post-school jobs which pay more (Meyer and Wise, 1982), and experience less unemployment (Johnston and Bachman, 1973; D'Amico and Baker, 1984a) following their leaving school than youth with no work experience. But concern has surfaced that high school employment may also be associated with increased delinquency and drug use (Bachman et al., 1981) and impaired school performance (Steinberg et al., 1982; but see D'Amico, 1984). These findings, coupled with recently voiced concern over declining standards of academic excellence (National Commission, 1983), call for a reevaluation of the advisability of high school employment.

Obviously, one desirable strategy would be to foster those outcomes of high school employment which prove to be valuable in the post-school period while attenuating or surpressing the undesirable latent effects. Perhaps, in fact, in-school substitutes for youth employment (e.g., improved job counseling, job training, etc.) could be found to provide alternate paths to the desirable outcomes. Devising policy directives along these lines is hampered by the lack of good evidence detailing what it is about high school employment that matters. In this paper we attempt to uncover reasons for the purported relationship between employment while in high school and post-school labor market outcomes. This exercise should enable us to suggest how work-study programs might be tailored to maximally benefit students. We will also examine the feasibility of in-school substitutes for youth employment.

I. REASONS WHY WORK MIGHT MATTER

At least two general explanations could be advanced for why employment during high school might ease the school to work transition--a general traits model and a specific skills model.

A. The General Traits Model

This model encompasses a variety of mechanisms, from general human capital development to credentialing. The human capital variant of the argument suggests that high school employment confers advantages in the form of information and skill acquisition which are valuable in subsequent labor market forays. Most simply, youth who work may acquire useful job skills and also knowledge about job opportunities and practice in job search and interviewing skills. School year employment also constitutes a form of preparatory socialization in that it inculcates appropriate work attitudes and behaviors. Strangely, both neoclassical and Marxist economists arrive at this same conclusion. Bowles and Gintis (1977) suggest that schools fulfill their most vital function by ingraining in youth attitudes of docility, punctuality, and willingness to follow orders--all habits which Edwards (1976) finds are strongly rewarded in the labor market. Thus, youth who work have desirable work habits doubly reinforced by being simultaneously immersed in school and work institutional settings. Under these circumstances, employers may waive their customary reluctance to hire youth just out of high school for positions of responsibility (Osterman, 1980).

Finally, under the rubric of the general traits model, favorable outcomes could result because high school employment acts as a screening or signalling device to prospective employers (Berg, 1971). Apart from whether or not early employment really does instill valuable work skills or attitudes, employers who lack clear-cut means to predict effective job performance use prior work history information to discriminate among potential job candidates.

B. The Specific Skills Model

Most jobs which employed students hold are low-skill positions which can be supposed to offer little in the way of general skill acquisition (D'Amico and Baker, 1984a). Rather, what is most useful may be information about the way the employing organization operates, or other firm specific skills which enhance one's productivity only so long as one remains with that employer. Alternatively, upon leaving school, already employed students may find their employers give them preferential consideration for permanent, post-school positions. For these reasons, only working youth who remain with the same employer upon leaving school could be expected to realize any advantages from their prior work experience.

The presupposition of this approach is that the higher post-school wages of working students observed in the literature are driven largely or solely by the higher wages of that subset of working students whose post-school employment and school year employment were with the same employer.

C. Choosing Between the Models

Understanding what it is about school year employment that matters rests on our ability to devise clear-cut empirical implications which differentiate the models. The specific skills model leads to the suggestion that the beneficial effects of school year employment apply to only a subset of youth who work. If that subset could be identified and isolated, one should find no effects in the sample of the remaining youth.

The specific skills argument presented above suggests that the benefits of school employment should be realized primarily by those whose post-school and school year employers were the same. These benefits should include a higher hourly wage in the post-school period because these youth reap the returns to firm specific human capital or other preferential consideration not available to those youth who either changed employers upon leaving school or who had no school year work experience. Similarly, youth who remain with the same employer by that fact show evidence that the outcome of their job match is more favorable to both the worker and the employer. Accordingly, one would expect less unemployment in the post-school period.

By contrast, the implication of the general trait model is that the benefits of school year employment are realized more generally, without regard to whether or not the worker changed jobs. In this case it should be impossible to identify any segment of working students for whom the effects of their work experience are greater; those who changed jobs as well as those who remain with the same employer should realize certain advantages over the members of their high school class who did not work while they were students.

Discriminating between variants of the general traits model is rather more difficult, because the implications of the various models are too similar. Nonetheless, some weak hypotheses can be formulated. If high school employment endowed youth with general skills which enhanced their productivity, then the surest effect is that youth who worked should have a higher hourly wage upon leaving school than youth with no work experience. Effects on the probability of experiencing unemployment are less certain, because youth who worked presumably have a higher asking wage and their schedule of job offers is indeterminant. On the other hand, if school year employment primarily acts as a screening device or inculcates desirable work habits, then these youth should be more successful in eliciting job offers, resulting in shorter spells of job search and less unemployment. Their hourly wage need not be enhanced, however, since no necessary effects on their productivity are implied.

In any case, the clearest case to be made is whether the general traits or specific skills model holds, and our primary method of discriminating between these models will be to discover whether the effects of school year employment hold for all youth or for only that subset who remain with the same employer.[1] After describing data and measurement methods we will test these implications.

II. DATA AND METHODS

Previous investigations of the effects of high school employment have been hampered by limitations of available measures of high school work experience. Johnston and Bachman (1973) rely upon total months worked, and Meyer and Wise (1982) must make do with a single question asking about usual hours worked while in high school. Neither study has the luxury of considering more refined measures of high school work intensity measures.

This study can utilize more refined measures of high school employment. Data for this study are taken from the 1979 to 1984 waves of the NLS youth cohort. All analyses are restricted to those enrolled in grades 10 through 12 in 1979, who graduated from high school and who did not attend college. Restricting the sample to terminal high school graduates introduces certain issues of sample selectivity bias and means that the effect of high school employment on the decision to attend college cannot be considered. However, this latter issue was dealt with in some detail elsewhere (D'Amico and Baker, 1984a) and at best only weak effects were found. Restricting the sample in this fashion has the advantage of focusing attention on a modal group of youth and ones whose post-school labor market experiences are likely to be most affected by high school employment. A previous chapter in this series (D'Amico and Baker, 1984b) examined in more general terms the school to work transition for terminal high school graduates, and the current paper should be viewed within the context of this earlier one. Of the original sample of over 12,000 cases, just under 2,000 met the various universe restrictions described above.

The high school work intensity variables used in this paper are measured for the respondent's last year in high school and are calculated from the extensive work history information collected in each survey year. In particular, start and stop dates, periods of interruption of employment, and usual hours worked are available from January 1, 1978 to the date of the 1983 survey for all jobs held that are "not odd jobs (but are) jobs done on a more or less regular basis."

A variety of work experience variables are created from these data, but all begin with essentially the same logic. First, each week the respondent was enrolled in high school during the senior academic year is identified.[2]

Then, work history data are used to ascertain during which of these weeks the respondent was employed. For each week the respondent was employed, usual hours worked per week on the job held during that week was assigned. The result is a sequence of week-by-week enrollment status and employment status variables for each respondent, identifying the weeks during the senior academic year in which the respondent was enrolled and for each week enrolled, the estimated hours worked during that week. Usual hours worked during the senior year is then created by summing hours worked each week and dividing by the total number of weeks enrolled.

III. THE EXTENT OF EMPLOYMENT AMONG TERMINAL HIGH SCHOOL GRADUATES

The available data permit a variety of ways of characterizing the work experience of high school youth. Several of these are presented in Table 6.1. This table presents information on the extent of work experience during grade 12. Data are reported by sex and minority status, with the minority status categories being minority group (black or Hispanic) and other (mostly white).[3]

In keeping with other evidence of the labor force participation rates of high school youth (e.g., Morgan et al., 1983; Lewis-Epstein, 1980), these data show both extensive and intensive work involvement amoung youth. Nearly 80 percent of white males worked at least one week during the school year and nearly 60 percent worked during at least half the weeks they were enrolled. Moreover, this work involvement is not incidental. Usual hours worked per week among white males is over 15 hours per week and 38.4 percent usually work more than 20 hours.

Other race/sex groups report levels of work invovlement not quite as high but still considerable. Next to white males, white females have the highest

Table 6.1 Extent of School Year Employment by Sex and Race for Terminal High School Graduates

	Females		Males	
Variables	Minority	White	Minority	White
Percent who worked:				
At least one week	47.4	71.5	59.8	78.3
Half the school year or more	32.4	57.0	41.0	59.3
Usual hours worked per week	7.2	12.8	10.3	15.6
Percent whose usual hours worked is:				
Didn't work	52.7	28.6	40.2	21.8
1 to 10 hours/week	17.5	21.2	20.5	20.3
10 to 20 hours/week	18.1	22.2	16.5	19.5
More than 20 hours/week	11.7	28.0	22.8	38.4
N	377	507	397	484

Universe: Students enrolled for at least 15 weeks as seniors during their last school year, who completed grade 12 and who did not attend college.

employment rates followed by minority males and minority females. Compared to similar measures of work intensity for all high school seniors (D'Amico, 1983), terminal graduates show just marginally greater levels of work involvement.

Restricting attention just to those seniors who worked, Table 6.2 shows an average hourly wage ranging from $3.87 for white males to $3.23 for minority females. Only males of both race groups show an average exceeding the minimum wage. Youth who work also tend to be stable in their work attachment, with the overwhelming majority working for no more than two employers during the academic year.

IV. THE EFFECTS OF EMPLOYMENT ON THE SCHOOL TO WORK TRANSITION

We gauge the effect of high school employment on improving the school to work transition in a multivariate context. Measures of school year employment, net of appropriate controls, are assessed for their effects on two outcome measures of post-school labor market success. These are: (a) hourly wage on the first survey after leaving high school (HROPYR1, 1983 dollars), and (b) the proportion of weeks unemployed divided by the number of weeks in the labor force, measured for the first 52 weeks after the date the respondent was last enrolled (PROPUN). Control variables include: (c) whether the respondent was Hispanic, or (d) black; (e) the respondent's father or other head of household's education (HEADED); (f) whether the respondent grew up in an intact home (i.e., if both mother/stepmother and father/stepfather were present at age 14); (g) the respondent's class rank, as a measured of ability (coded in three categories: RANKHI, upper quartile; RANKLO, lower quartile; and RANKNA, rank not available; the middle 50 percent in the left out category); (h) whether the respondent was married or (i) had a child by the

Table 6.2 Nature of School Year Employment by Sex and Race for Terminal High School Graduates Who Worked in High School

	Females		Males	
Variables	Minority	White	Minority	White
Usual hours worked per week	15.3	17.9	17.3	20.0
Percent whose usual hours worked is:				
1 to 10 hours/week	37.0	29.7	34.3	26.0
10 to 20 hours/week	38.2	31.1	27.6	24.9
More than 20 hours/week	24.8	39.3	38.1	49.1
Average hourly wage[a]	3.23	3.32	3.58	3.87
Number of employees				
1	42.9	37.8	50.8	35.0
2	43.7	41.5	37.1	47.0
3 or more	13.5	20.6	12.2	18.0
N	183	354	238	372

Universe: Students enrolled for at least 15 weeks as seniors during their last school year, who completed grade 12 and who did not attend college and who worked at least one week during their senior year.

[a] All hourly wage figures are in 1983 dollars.

first survey after leaving school; and (j) for the hourly wage equation only, the number of weeks of work experience from the week the respondent left school to the date the post-school hourly wage was measured (WKDLEYR1). Means on these variables are presented in Table 6.3. Regression results are presented separately by sex given that even young males and females are known to compete in different labor markets (Greenberger and Steinberg, 1983).

The strategy adopted is to measure high school employment in progressively more complicated ways to capture more precisely what it is about the work experience that matters. Results for the unemployment equations (PROPUN) are presented in Tables 4 and 5, for females and males, respectively, with the hourly wage results (HROPYR1) presented in Tables 6 and 7. One complication must be addressed at the outside. For the unemployment equations, some youth have no weeks in the labor force; therefore PROPUN is undefined. In fact, any respondent with less than 20 weeks in the labor force in the 52 weeks after leaving school is omitted from the PROPUN equation to minimize instability of results resulting from poor measures of post school ease in adjusting to employment for those with minimal labor force involvement. Similarly, in the hourly wage equations, some youth were not employed on the first survey after leaving school and hence have no measureable HROPYR1. To correct for selectivity bias resulting from the exclusion of these respondents from the regression results, Olsen's (1980) sample bias correction method was utilized. Results of these selectivity equations are presented in Appendix Tables 6A.1 and 6A.2, and resultant selection factors are included in each of the PROPUN and HROPYR1 equations.

The estimation of the effects of school employment on proportion of weeks unemployed, shown in Tables 6.4 and 6.5, begins with measuring school year employment by a simple dummy variable (WRKINHS) which takes on the value "1"

Table 6.3 Variable Descriptions and Means for Variables Used in Regression Analysis, by Sex

Variable	Description	Means Female	 Male
Dependent variables			
HROPYR1	Hourly wage at first survey past high school (1983 dollars)	3.98	4.95
HROPYR3	Hourly wage at third survey past high school (1983 dollars)		
PROPUNEM	Weeks unemployed in 52 weeks after high school/weeks in labor force	.16	.16
Key independent variables			
WRKINHS	Did R work in senior year (1 = yes, 0 = no)	.79	.81
SRUSHR	Usual hours worked in senior year	14.8	17.2
SRUSHRSQ	Usual hours squared	380.2	486.4
SAMEMP	Is R's first employer after high school same as high school employer		
DIFFEMP	Is R's first employer after high school different from high school employer		
Control variables			
INTACT	R lived with father and mother at age 14	.89	.86
WKSWRKY1	Weeks worked from date last enrolled to first survey	30.9	32.0
HISPANIC	R is Hispanic (1 = yes, 0 = no)	.05	.05
BLACK	R is black (1 = yes, 0 = no)	.08	.11
RANKLO	R is lower quartile in class rank	.16	.29
RANKHI	R is upper quartile in class rank	.16	.07
RANKNA	Class rank is NA	.25	.33
MARRYR1	Marital status first year out	.13	.06
CHILDYR1	Number of children first year out	.05	.04
HEADED	Head of household's education	11.2	11.2
MARRYR3	Marital status third year out		
CHILDYR3	Number of children third year out		

Note: Sample sizes and means of all independent variables are from the HROPYR1 equation using leastwise deletion. Variables are defined more fully in the text.

Table 6.4 Regression Analysis of Effects of High School Work Experience on Percent of Weeks Unemployed After Leaving High School, for Females

	Dependent variable: PROPUN					
	Equation 1		Equation 2		Equation 3	
Independent variables	b	s.e.	b	s.e.	b	s.e.
BLACK	.023	.046	.079	.053	.070	.052
HISPANIC	-.162**	.060	-.113*	.064	-.119*	.063
INTACT	-.033	.034	-.036	.034	-.037	.034
HEADED	-.010**	.004	-.011**	.004	-.011**	.004
MARRYR1	-.167**	.051	-.071	.069	-.069	.068
CHILDYR1	-.231**	.064	-.123	.082	-.145*	.081
Sample selection	-.762**	.192	-.329	.282	-.403	.278
WRKINHS	-.092**	.029	-	-	-	-
SRUSHR	-	-	-.009**	.003	-	-
SRUSHRSQ	-	-	.0001**	.000	-	-
SAMEMP * SRUSHR	-	-	-	-	-.018**	.004
SAMEMP * SRUSHRSQ	-	-	-	-	.0003**	.0001
DIFFEMP * SRUSHR	-	-	-	-	-.005*	.003
DIFFEMP * SRUSHRSQ	-	-	-	-	.000	.000
Constant	.318	.063	.370	.069	.351	.068
R^2	.144		.150		.178	

Note:

Table 6.5 Regression Analysis of Effects of High School Work Experience on Percent Weeks Unemployed After Leaving High School, for Males

	Dependent variable: PROPUN					
	Equation 1		Equation 2		Equation 3	
Independent variables	b	s.e.	b	s.e.	b	s.e.
BLACK	.063*	.036	.090**	.042	.072*	.041
HISPANIC	-.035	.051	.006	.062	-.004	.061
INTACT	-.036	.030	-.036	.029	-.026	.029
HEADED	-.009**	.004	.010**	.004	-.011**	.004
MARRYR1	-.063	.047	-.086*	.050	-.096**	.049
CHILDYR1	.072	.050	.034	.058	.058	.056
Sample selection	-.815**	.247	-.192	.573	-.424	.560
WRKINHS	-.040	.030	-	-	-	-
SRUSHR	-	-	-.004	.003	-	-
SRUSHRSQ	-	-	.000	.000	-	-
SAMEMP * SRUSHR	-	-	-	-	-.010**	.003
SAMEMP * SRUSHRSQ	-	-	-	-	.0001**	.000
DIFFEMP * SRUSHR	-	-	-	-	.0008	.003
DIFFEMP * SRUSHRSQ	-	-	-	-	-.000	.000
Constant	.271	.058	.333	.080	.306	.079
R^2	.120		.121		.169	

Note:

if the respondent worked at all as a senior, and "0" otherwise. Results, in equation 1 of these tables, show the dummy variable is significant for females but not for males. For females having worked reduces the proportion of weeks unemployed by nine percentage points. Equation 2 refines our measure of work involvement by substituting usual hours worked while a senior (SRUSHR) and usual hours squared (SRUSHRSQ). Incorporating these measures permits an assessment of whether the effect of school year employment is sensitive to the amount of work experience and, if so, whether a threshold is reached beyond which additional hours of senior year employment cease to matter. Results for females shows that incorporating these measures of work intensity into the equation improves fit over merely measuring whether the respondent worked or not (R^2 of .150 vs. .144 for equation 1) and that hours worked decreases unemployment at a decreasing rate. For males, once again, no effect of high school work involvement is observed.

The above equation still does not allow us to differentiate between the general traits and the specific skills models. To do so we introduce equation 3, which adopts an analysis of covariance approach by including in the equation separate measures of hours worked in high school and hours worked squared for those with the same employer before and after leaving school (SAMEMP) and those who changed employer (DIFFEMP).[4] If the magnitude of the effect of school year employment is the same for both these groups, support for the genral traits model would be inferred. If on the other hand the effects appeared only for those with the same employer, we would conclude that the specific skills model was supported.

Results for females show the coefficient for those with the same employer, when calculated over any reasonable range of usual hours, is much larger in absolute value than the corresponding coefficient for those who changed employers. There is a marginally significant effect of school employment for those who changed employers, suggesting some benefit of having learned general traits among all those who worked. But those who worked with the same employer are much less likely to experience unemployment than either those who did not work or those who worked but changed employers. Moreover, finally for males significant effects of high school employment emerge, but these only for those who did not change employers. In short, support seems greatest for the specific skills model, at least insofar as weeks of unemployment is concerned. While females may reap some general benefits from having worked in high school, males and females who remained with the same employer find their prospects for a smooth transition to work much improved.

In contrast to these results, those for the hourly wage equations, presented in Tables 6.6 and 6.7, provide no evidence whatever that high school employment is advantageous. Whether measured as a dummy or continuous variable, and whether measured separately for those who remained with their same employer, there is no evidence that those who worked in high school have a higher hourly wage after leaving school. In fact, there is some evidence that female students who worked, especially those who changed employers, will have a somewhat lower wage in their post-school job. At first glance, this finding of no effect is surprising and runs counter to conventional wisdom. But in fact the effects observed by others on the hourly wage in the year after leaving school have been at best small and often sporadic (e.g., D'Amico and Baker, 1984a; Meyer and Wise, 1982). We must conclude that the evidence that employment during high school improves one's post-school wages seems uncertain.

Table 6.6 Regression Analysis of Effects of High School Work Experience in Hourly Wage the First Year After Leaving High School, for Females

Independent variables	Dependent variable: PROPUN					
	Equation 1		Equation 2		Equation 3	
	b	s.e.	b	s.e.	b	s.e.
BLACK	.389	.276	.762*	.443	.621	.447
HISPANIC	.260	.306	.352	.317	.321	.316
INTACT	.216	.210	.217	.209	.156	.210
HEADED	.051**	.025	.044*	.025	.042*	.025
MARRYR1	.402*	.233	.793*	.449	.732	.449
CHILDYR1	-.097	.315	.218	.461	.100	.465
RANKHI	-.303	.190	-.377*	.210	-.347*	.210
RANKLO	-.120	.185	-.055	.192	-.081	.192
RANKNA	.202	.166	.327	.204	.295	.205
WKSWRKYR1	.018**	.010	.015**	.007	.016**	.007
Sample selection	2.023**	.659	3.803*	2.036	3.202	2.054
WRKINHS	-.006	.189	-	-	-	-
SRUSHR	-	-	.016	.023	-	-
SRUSHRSQ	-	-	-.0010**	.0004	-	-
SAMEMP * SRUSHR	-	-	-	-	-.007	.025
SAMEMP * SRUSHRSQ	-	-	-	-	-.000	.001
DIFFEMP * SRUSHR	-	-	-	-	.041	.025
DIFFEMP * SRUSHRSQ	-	-	-	-	-.001**	.000
Constant	3.188	.495	3.837	.920	3.649	.926
R^2	.085		.096		.105	

Table 6.7 Regression Analysis of Effects of High School Work Experience on Hourly Wage the First Year After Leaving High School, for Males

	Dependent variable: PROPUN					
	Equation 1		Equation 2		Equation 3	
Independent variables	b	s.e.	b	s.e.	b	s.e.
BLACK	-.289	.425	-.008	.556	-.183	.558
HISPANIC	.011	.561	.113	.575	.097	.574
INTACT	.154	.348	.173	.350	.251	.350
HEADED	.045	.045	.048	.045	.033	.045
MARRYR1	.058	.525	.090	.528	.067	.528
CHILDYR1	4.267**	.605	4.284**	.606	4.336**	.603
RANKHI	-.721	.529	-.998	.652	-.817	.652
RANKLO	-.402	.301	-.417	.302	-.424	.300
RANKNA	-.204	.293	-.145	.300	-.114	.299
WKSWRKYR1	.002	.013	-.003	.014	.005	.014
Sample selection	1.941	1.310	4.625	3.752	3.153	3.791
WRKINHS	-.274	.371	-	-	-	-
SRUSHR	-	-	-.029	.035	-	-
SRUSHRSQ	-	-	.000	.000	-	-
SAMEMP * SRUSHR	-	-	-	-	-.046	.036
SAMEMP * SRUSHRSQ	-	-	-	-	.000	.001
DIFFEMP * SRUSHR	-	-	-	-	-.015	.044
DIFFEMP * SRUSHRSQ	-	-	-	-	.000	.001
Constant	5.018	.967	5.998	1.668	5.327	1.691
R^2	.100		.100		.113	

IV. CONCLUSION

This paper adds its weight of evidence to other studies which show now pervasive is the employment of high school youth. It is safe to say that, from being a relative rarity, most youth work during their senior year of high school and, among those who work, weekly hours of employment exceeding 15 or 20 are common. With students exhibiting this degree of employment intensity, it is natural to question whether youth have lost sight of their primary role obligation as students. While there are good reasons for encouraging some youth employment (e.g., National Panel, 1974), well-founded concern over declining standards of academic excellence (National Commission, 1983) should cause a reappraisal of whether in fact high school employment engenders the desirable outcomes originally foreseen or whether these advantages come at too high a cost. Recent Presidential initiatives to foster greater youth employment through a lower minimum wage make this agenda even more urgent.

A basic lack of information regarding the concomitants of high school employment has only begun to be redressed. Much of the recent research, however, has produced contradictory findings. Steinberg et al. (1982) caution against youth employment in light of their finding that work while in high school retards academic achievement; D'Amico and Baker (1984) finds no such reason for alarm. Meyer and Wise (1982) find that high school youth who worked as sneiors have higher wages than comparable non-working students even four years after leaving school; this study, meanwhile, finds no effects on hourly wage for males or females even one year after graduation. This conclusion holds equally for youth who changed employers once they left school as it does for youth with the same employer.

If any one finding is consistent across a multitude of studies, however, it is that youth who worked as students have an easier time finding a job and experience less unemployment once they leave school (D'Amico and Baker, 1984b; Meyer and Wise, 1982). This study delves further into this finding to uncover why student employment should have this effect. We find the effect prevails primarily among those youth who worked and stayed with the same employer after graduation. Various explanations described under the specific skills rubric would predict such selective effects of youth employment--the acquisition of firm specific human capital, priority in promotion queues, and so on. But in light of the absence of effects on hourly wage, that explanation seems best which is in many ways the simplest. Some youth who work as seniors do have an easier time making the school to work transition because they already have a post-school employer lined up and hence are spared the necessity of prolonged job search following graduation. Evidence that youth who work acquire general or firm-specific job skills, are screened differently by employers, or learned useful job attitudes or behaviors seems scanty at best. Moreover, it is not clear how long-lasting even the effects on subsequent unemployment are. Youth who worked in high school and continue working for their same employer may only have deferred and not eliminated the need for job search since they may subsequently endeavor to secure a new position offering greater career possibilities than their erstwhile high school job could provide.

None of this analysis should be taken to mean that high school employment should be discouraged or that it offers few benefits. Possibly greater attention to job counseling in high school could facilitate post-school job matching for students who chose not to work. But employment may offer a host of additional benefits--fostering independence or initiative, for example--which recent research efforts are only beginning to uncover.

Chapter 6

Notes

[1] Our ability to test between these models is really not as definitive as this statement makes it appear. A specific skills model may prevail but go undetected in this analysis because we misidentify the subset that benefits. Rather than have youth with the same employer realize benefits, it may be that only youth in certain industries or occupations learn useful skills, and these may be rewarded regardless of whether they cahnge employers or not. Greenberger et al. (1982) notes, for example, that while most youth jobs are low skill, a small number do impart useful skills and impart desirable personality traits. Alternately, we may find support for the specific skills model, but misidentify the reason. Rather than certain jobs imparting valuable job skills, it may be that some students procured employment while still in school with employers who paid high wages for whatever reason (e.g., unionization). Such students would be more likely than others to remain with their employer upon leaving school, giving rise to false support for the specific skills model.

[2] For purposes of this paper the senior academic year is defined as beginning on September 15 and ending on May 15 of the following year, encompassing the last period during which the respondent identified him or herself as being in grade 12. Prior to the 1980 interview date, weekly enrollment status is gauged by a question asking whether the respondent is currently enrolled, and, for those answering "no," the date last enrolled. For these survey years, enrollment status is assumed to be continuous from 9/15 of the preceding year to the interview date (interviews are generally conducted in the first few months of each year) for those not currently enrolled but enrolled sometime since the start of the school year. Moreover, for the first group, unless the subsequent year's interview reports a date last enrolled prior to 5/15 of the previous year, enrollment status is further assumed continuous to 5/15. Subsequent to the 1980 interview date, a monthly school attendance variable is available for each respondent. The school year is still defined as beginning on 9/15 and ending on 5/15 of the subsequent year, but the monthly enrollment status variable enables month-long interruptions in school attendance to be registered for each respondent.

[3] Hispanics are not included in the "whites" category because of their status as a minority group. Combining blacks and Hispanics into a single category doubtless masks some important differences between these two groups, but small sample sizes preclude presenting results separately for each of them. Moreover, preliminary results showed the two groups to be more like each other in the processes under examination here than either group is like "whites."

[4] SAMEMP is defined to equal "1" if the last employer the respondent had while still a student was the same as the first employer the respondent had after leaving school, and "0" otherwise.

Chapter 6

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Table 6A.1: Sample Selection Regression for the PROPUN Equation, by Sex

	Dependent variable: in labor force greater than or equal to 20 weeks[a]			
	Female		Male	
Independent variables	b	s.e.	b	s.e.
BLACK	-.136**	.041	-.042	.031
HISPANIC	-.119**	.061	-.063	.046
MARRYR1	-.215**	.035	.035	.048
CHILDYR1	-.248**	.041	.057	.049
EDUCEXP[b]	.016*	.008	.003	.006
RANKHI	-.016	.040	.022	.045
RANKLO	-.081**	.038	-.008	.026
RANKNA	-.034	.033	-.034	.025
SRUSHR	.004**	.001	.003**	.001
Constant	.702	.112	.867	.083
R^2	.214		.057	

[a] The dependent variable is coded '1' if the respondent was in the labor force for at least 20 weeks in the 52 weeks after leaving school, and '0' otherwise.

[b] Educational expectations, measured as the number of years of schooling the respondent expects to complete.

Table 6A.2 Sample Selection Regression for the HROPYR1 Equation, by Sex

	Dependent variable: working YR1[a]			
	Female		Male	
Independent variables	b	s.e.	b	s.e.
BLACK	-.188**	.045	-.017	.040
HISPANIC	-.061	.066	-.035	.065
MARRYR1	-.203**	.040	-.022	.065
CHILDYR1	-.180**	.044	-.033	.075
EDUCEXP[b]	.007	.009	-.003	.008
RANKHI	.053	.046	.108	.066
RANKLO	-.032	.043	.000	.036
RANKNA	-.072**	.036	-.024	.035
WKDLEYR1[c]	.004**	.002	.005**	.002
SRUSHR	.010**	.001	.008**	.001
Constant	.389	.141	.481	.133
R^2	.185		.081	
N				

[a] Coded '1' if the respondent was working at the time of the first survey after leaving school, and '0'otherwise.

[b] Educational expectations, measured as the number of years of schooling the respondent expects to complete.

[c] Number of weeks from the date last enrolled in school to the first survey after leaving school.

Chapter 7

Wage Determination in the Post-Schooling Period: The Market for On-the-Job Training

by

Donald O. Parsons*

I. INTRODUCTION

The years following completion of formal schooling are economically important ones for male youths. Average wage rates and earnings rise rapidly in this period. For example, among males in the youth cohort of the National Longitudinal Surveys who left school in the year preceding the initial interview in 1979, the average hourly wage rate increased from $4.01 in 1979 to $6.08 in 1982 (Table 7.1). The implied annual growth rate of 15.0 percent is double that for the hourly wages of production and nonsupervisory workers of all ages (U.S. Bureau of Labor Statistics, 1983). This rapid growth rate in the immediate post-schooling period is characteristic of young workers at all levels of attained schooling (Table 7.1).

The process that generate these large average increases in wage rates among youth in the immediate post-schooling period is clearly important in the functioning of the labor market. The process is crucial as well to issues of income distribution since the increases are far from uniform across individuals. The period is partcularly important since the differences in

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Table 7.1 Post-Schooling Hourly Wage Rate of Male Youth in Total and by Schooling Attainment, 1979-1982[a]

	Year since school departure				
	(1) First (1979)	(2) Second (1980)	(3) Third (1981)	(4) Fourth (1981)	Annual growth rate 1979-1982
Total	$4.01	$4.64	$5.43	$6.08	14.7%
Schooling					
S less than 12	3.39	3.88	4.62	5.03	14.1%
S = 12	4.19	4.93	5.65	6.46	15.5%
S greater than 12	4.89	5.56	6.61	7.56	15.6%

[a] Civilian males out of school in 1979, but enrolled in 1978 or 1979 (prior to first interview). Individuals with estimated wage rates less than $0.50 or greater than $25.00 were deleted from the sample. Sample size: 1979, total schooling (458), S less than 12 (161), S = 12 (235), S greater than 12 (62); 1980, total schooling (544), S less than 12 (197), S = 12 (269), S greater than 12 (78); 1981, total schooling (548), S less than 12 (190), S = 12 (2.79), S greater than 12 (79); 1982, total schooling (606), S less than 12 (221), S = 12 (304), S greater than 12 (81).

earnings that emerge at this stage become quite stable as the individuals mature. The earnings of white males 45 to 54 years of age with 12 years of schooling, for example, have an autocorrelation coefficient of 0.77 over a 5-year interval. With 16 years of schooling the autocorrelation of earnings over a 5-year interval is 0.89 (Parsons, 1978). The distribution of adult incomes and life time human wealth across individuals is therefore strongly dependent on the process underlying the early, more fluid period of labor market activity.

Because of the obvious implications of this early wage growth process for the distribution of earnings and income, this period in the life cycle has been the focus of a number of large-scale government job programs designed to increase the rate of skill development among the least successful youth. The design of these programs are greatly hindered, however, by the lack of information and understanding of the private market processes which generate the bulk of the economy's training opportunities and with which the government training programs must coordinate. Indeed, a major innovation of the most recent jobs programs (JTPA) is an attempt to increase the flow of private market information in the design and operation of government subsidized training programs. Nonetheless, a better general understanding of how youth are trained in the private market would surely be invaluable in the structural design of such programs. This chapter is a contribution toward that goal. It is based on a data set uniquely designed for this purpose.

Hypotheses other than on-the-job training have been proposed as models of the process underlying early wage growth, e.g., learning by workers about job possibilities through purposeful job search (Burdett, 1978) and learning by firms about workers so that the workers can be more effectively utilized within the firm (Ross, Taubman, and Wachter, 1980). Empirically the contribution of job mobility to early wage growth appears to be modest (Bartel, 1975; Mincer and Jovanovic, 1980). The firm learning hypothesis is rather more difficult to confront empirically but lacks the intuitive plausibility of the on-the-job training hypothesis. The on-the-job training hypothesis will therefore be the principal focus of the analysis that follows.

In its simplest form, the on-the-job training hypothesis is no more than an extension of schooling choice into a period of incomplete specialization in education (Mincer, 1962, 1974; Ben-Porath, 1967). Rosen (1972), however, has stressed the unique supply characteristc of training at the workplace and the resulting transactions and "prices" that will be established in this situation. Specifically, Rosen proposes that individuals be viewed as choosing among jobs based on a compensation bundle that includes both current wages and on-the-job training to be included in a particular job, based on the technological characteristics of the job and the firms' own efficiency at providing such training. The interaction of these individual and employer choices yields the equilibrium level of on-the-job in the economy as well as the rate of return to the activity.

The NLS youth sample provides an excellent opportunity for the closer examination of the wage/employment process in the immediate post-schooling period. In this paper I examine the post-schooling experience of a subsample of the youth survey, namely civilian males who were not enrolled in school at the time of the first survey in early 1979 but who reported being enrolled in school within the last year (1978 or 1979). Even with these restrictions the sample size is approximately 600 because of the large size of the total sample. The individuals in this subsample are disproportionately high school dropouts and high school graduates because of the age restriction on the initial sample, 14 to 22 in 1979. The labor market experience of the subsample is followed over the early post-schooling period 1979 to 1982.

The NLS youth survey has many features that make it an ideal sample for this undertaking. The labor market experience of the youth is recorded in unusual detail, detail only possible with the annual reinterviewing design of the data set. Moreover, virtually all respondents took an extensive set of tests to measure the skills they bring to the labor market, namely, the Armed Services Vocational Aptitude Battery (ASVAB). Components of the test include general science, arithmetic reasoning, word knowledge, paragraph comprehension, numerical operations, coding speed, automotive shop information, mathematics knowledge, mechanical comprehension, and electronics information.

Perhaps most importantly for the analysis of on-the-job training, each respondent is asked for his perception of the learning content of his job. The exact nature of this variable will be defined more carefully below. This variable offers the possibility of a direct examination of the on-the-job training hypothesis rather than one based on earnings growth or a residual analysis of earnings. It is possible to explore the impact on subsequent labor market success not only of holding a job (Ellwood, 1982) but of the training quality of the job.

A number of issues are considered below. Perhaps the most important is the impact of labor market experience on subsequent wage rates of the worker. What impact does having a job have on the wage growth process? Does the perceived learning content of the job matter or is the holding of a job itself valuable through the development of work disciplines, etc.?

Market equilibrium aspects of on-the-job training are also explored. The unrestricted market model of Rosen, for example, suggests that "equalizing or compensating differentials" should exist so that individuals in high learning jobs with the prospect of higher wages in the future would receive lower current wages much as individuals in formal schooling must forego current income. This proposition is an empirical one since a variety of institutional forces support wage rates above those that might otherwise prevail in a competitive labor market, e.g., the minimum wage (Fleisher, 1981; Leighton and Mincer, 1981; Hashimoto, 1982).

The responses to the on-the-job learning question permit analysis of the local and availability of jobs with high learning content as well as analysis of its impact on wages. Not surprisingly, high learning jobs are relatively easy to characterize; they are jobs that require disproportionate levels of mental, physical, and social skills. Among male youth in 1982 who had jobs and were not enrolled in school, approximately two-thirds of all professionals, managers, and craftsmen report that their jobs have high learning content. In contrast, only one-third of laborers and service workers and only 25 percent of farm laborers reported that their jobs had high learning content. (See Table 7.2).

Large differences in job learning content are also found across industries, with youth in the finance, mining, and construction sectors reporting high learning experiences (See Table 7.3). Conversely, in agriculture only 28 percent report high learning jobs, and in wholesale and retail, only 39 percent. A multivariate logit analysis, however, indicates that most of these industrial differences are a reflection of differences in the occupational mix of the work forces in the various industries.

A more interesting social question is the allocative one, which youth secure jobs with high learning content. The process may be quite different from the decision to enter college since firms may be sharing in the job training investment (Parsons, 1984) and may be selecting trainees in a manner that stresses job stability, etc. (Lester, 1954; Bishop, 1985) as well as

Table 7.2 The Probability of Holding a High Learning Job by Occupation, Males with a Job and Not Enrolled in School, 1982

Occupation (1)	Probability of a High Learning Job (%) (2)	Sample Size ( )
Professional	70.3	145
Managerial	62.9	124
Craftsman	61.1	427
Sales	47.3	91
Operative	39.7	634
Clerical	39.7	229
Laborer	36.5	318
Service worker	33.9	336
Farm laborer	25.4	59
Total	45.4	2363

Source: Youth Survey of NLS

Table 7.3 The Probability of Holding a High Learning Job by Industry, Males with Job and Not Enrolled in School, 1982

Industry (1)	Probability of a High Learning Job (%) (2)	Sample Size (3)
Finance	64.6	65
Mining	62.1	66
Construction	58.5	236
Public Administration	48.9	88
Transportation	47.1	157
Service, Entertainment	43.2	88
Manufacturing	42.7	616
Professional	41.9	155
Wholesale and Retail	39.4	592
Agriculture	28.3	106
Total	44.6	2169

Source: Youth survey of NLS.

general aptitude. Do the high learning jobs go primarily to the most able school leavers? What role is played by parental characteristics, e.g., family wealth and schooling? A logit analysis of the likelihood of securing a high learning job is undertaken and the estimates compared with the factors that influence the college enrollment decisions of high school seniors.

Finally, the intracohort distribution of jobs, particularly jobs with high learning content, is explored. If the year-to-year likelihood of holding a job with high learning content (or low) is high, the consequent distribution of wage rates will have greater variance and be more skewed than if the holding of such jobs fluctuates more randomly across individuals over time. Moreover, if compensating differentials in wages across jobs with different learning content are small or nonexistent, the access to such jobs will have consequences for the distribution of lifetime wealth as well as annual income in later years.

II. THE MARKET FOR ON-THE-JOB TRAINING

As Mincer (1962, 1974) first argued, an individual confronting jobs with varying prospects of learning and skill development faces a human capital investment decision identical in form to the schooling choice. The individual should be willing to accept lower current wages for jobs that offer greater learning prospects and consequently the prospect of higher future earnings. Indeed, in a competitive labor market with equal access to capital markets, the wages for higher learning jobs should fall to levels that equalize the present values of income streams across alternative career paths. This proposition on human wealth equalization is imposed by competition among workers in the labor market and is not dependent on the supply of high learning jobs that exist in the economy.

The extent of on-the-job training in an economy is determined by the supply of on-the-job prospects. To some extent these learning prospects are an intrinsic part of particular labor market activities. Industries differ widely in production technologies as do specific jobs within industries. Some job activities offer considerable complexity of task and interaction with others and require adaptation to changing circumstances; others do not. Other on-the-job learning opportunities are the result of conscious planning and commitment of resources by the firm (Rosen, 1972) and may be augmented or reduced in scope as economic prospects warrant. In a world of perfect capital markets and competitive labor markets, however, the present values of earnings streams will be equalized.

The market for on-the-job training is more complex if wages are institutionally constrained to exceed some minimum. If the skills created are generally demanded in the economy, yet institutional constraints limit the extent to which wages can correspondingly be adjusted downward during the training period, then the nature of the supply of learning prospects becomes crucial. If the learning activity is the result of (avoidable) resource commitments by the firm, presumably the resources will be withdrawn and learning opportunities reduced (Fleisher, 1981; Leighton and Mincer, 1981; Hashimoto, 1982). If the skills are specific to the firm and its own production process, presumably the activities may continue with the firm bearing a larger share of costs and securing a larger share of returns. If the skills created are general and the supply of learning possibilities intrinsic to the activity, then effective wage minimums will result in windfalls to those workers fortunate enough to secure the positions. Those individuals who secure the high learning jobs will have greater lifetime wealth than those who do not.

The possibility that high learning jobs may not induce equalizing differentials but rather generate human wealth inequalities is a real one. For young men in the youth cohort who left school within a year of the first survey in 1979, approximately 30 percent had wage rates less than the prevailing minimum wage in the (effective) covered sector of $2.90 per hour. In the large firm sector of the economy, moreover, trade unions, the threat of trade unions, and various informational and monitoring difficulties may induce yet higher effective minimums (Garen, forthcoming).

III. ON-THE-JOB TRAINING AND WAGE RATES

In this section estimates are presented of the impact of on-the-job training on wage rates of post-school youth. The discussion above suggests that on-the-job training will affect wage rates in two ways: (1) subsequent wages should be higher, reflecting the increased skills, and (2) current wages may be reduced, reflecting a compensating differential, the worker's payment for the learning opportunity.

The empirical analysis is based on a sample of males drawn from the youth survey of the NLS, specifically males who were not enrolled in school at the time of the first survey in early 1979 but who reported that they had been enrolled in school either in 1978 or 1979. The longitudinal aspect of the NLS permits a careful tracking of the work experience of these youth. The principal analysis will focus on the impact of that job experience, or lack of experience, on hourly wage rates in 1982. Because of the age limitation on survey participants, 14 to 22 years of age in 1979, college level job leavers are underrepresented in the sample.

Since the data are set rather firmly in one specific time interval, 1979 to 1982, the economic environment in that time period warrants brief discussion. In Table 7.4, statistics are presented for the performance of the U.S. economy as a whole between 1979 and 1982. The period was a difficult one for entry into the labor market. The male unemployment rate rose sharply, from 5.1 percent to 9.9 percent, with much of the increase coming late in the period with the 1982 recession. The experiences of the group under study may have been strongly influenced by this difficult entry point and may not be typical of entrants under all business conditions.

As noted earlier, the legal minimum wage was a factor of potential consequence for this sample primarily composed of high school dropouts and high school graduates who recently completed schooling. Again approximately 30 percent of the sample who had jobs in 1979 reported wages less than the legal minimum wage. Although average wage rates increased faster than the legal minimum wage, the wage growth was not uniform across individuals in the sample so that the percentage of the sample with wage rates at or below the minimum wage remained at approximately 30 percent. The legal minimum wage may well have imposed an effective floor on the wage rates of trainees in the covered section.

The principal analysis in this section is based on a regression analysis of the log of the hourly wage rate in 1982 of individuals in the sample who were employed at the time of the survey. A number of explanatory variables found in standard log wage analyses are included here: age, race and ethnic background (Hispanic or not), schooling, and presence of collective bargaining at the workplace. The individuals in the sample were also subjected to an extensive battery of aptitude tests, the Armed Services Vocational Aptitude Battery (ASVAB), which is composed of 10 subtests. The 10 subtests are arithmetic reasoning, mathematical knowledge, word knowledge, paragraph comprehension, general science, electronics information, numerical operations,

Table 7.4 Economic Statistics for U.S. 1979-1982

	1979	1980	1981	1982
Civilian unemployment rate				
Aggregate	5.8%	7.1%	7.6%	9.7%
Male	5.1%	6.9%	7.4%	9.9%
Inflation rate[a]	11.3%	13.5%	10.4%	6.3%
Legal minimum wage	$2.90	$3.10	$3.35	$3.35

[a] The percentage change in the aggregate consumer price index from prior year.

Sources: Unemployment rate and consumer price index, Handbook of Labor Statistics (1983); minimum wage level, Statistical Abstract of the United States 1984.

coding speed, auto shop information, and mechanical comprehension. Because of the high intercorrelation of a number of tests, the 10 scores were collapsed into 5 variables, MATH, VERBAL, SCIENCE, NUMERICAL, and MECHANICAL, each of which is the sum of 2 test scores (in order above). These scores would seem to provide a relatively complete set of indicators of the intellectual abilities the individuals bring to the workplace. The schooling coefficient therefore, if significant, is likely to represent a school certification effect.

The information on job experience is extensive. In this paper I consider the job characteristics of the job held, if any, at each of the annual survey dates. The NLS offers more detailed information which could be exploited if the initial results appear to warrant it. The measurement of actual work experience is, of course, a substantial improvement over customary single period cross-section analyses which use experience variables such as (age minus schooling minus 6).

Of particular interest here is the information on the respondent's perception of the learning quality of his job if he held one. Individuals with jobs were asked to respond "very true," "somewhat true," "not too true," or "not at all true" to the statement: "The skills you are learning would be valuable in getting a better job." Separate one-zero dummies were created for each of the four responses for those who held jobs at the survey date. The dummies are labelled HIGH LEARN, MODERATE LEARN, LOW LEARN, and NO LEARN below.

The Log Wage Regression Results. The log wage regression results are reported in Table 7.5 for log wage 79 and log wage 82 (columns 1 and 2). At this point it is the log wage 82 results in column 2 that are most interesting. Before turning to the estimates of the job experience effects,

Table 7.5 Log Wage Regression Results, 1979 and 1982, Recent Male School Leavers in 1979[a]

	(1) 1979	(2) 1982
Constant	-0.222	0.691
	(0.91)	(2.09)
Race and ethnic		
Black	-0.099	-0.010
	(1.93)	(0.18)
Hispanic	-0.116	0.115
	(1.39)	(1.29)
Age	0.049	0.003
	(2.52)	(0.13)
Schooling	0.047	0.034
	(2.41)	(1.73)
ASVAB scores		
Math	0.001	-0.002
	(0.25)	(0.28)
Verbal	0.005	0.011
	(0.74)	(1.46)
Science	-0.023	-0.022
	(2.45)	(2.09)
Numerical	0.001	0.0003
	(0.34)	(0.12)
Mechanical	0.014	0.022
	(2.35)	(2.99)
Current job (t)		
High learn	0.024	0.142
	(0.46)	(1.64)
Moderate learn	0.049	0.051
	(0.90)	(0.59)
Low learn	-0.058	0.036
	(1.00)	(0.38)
No learn	-	-
Collective bargaining	0.260	0.286
	(6.03)	(6.23)
Past employment status		
High learn job (t-1)	-	0.106
		(1.84)
Moderate learn (t-1)	-	0.121
		(2.07)
Low learn (t-1)	-	0.044
		(0.61)
No learn (t-1)	-	0.034
		(0.41)
No job (t-1)	-	-
High learn (t-2)	-	0.132
		(2.41)

Table 7.5 (continued)

	(1) 1979	(2) 1982
Moderate learn (t-2)	-	0.122 (2.11)
Low learn (t-2)	-	0.152 (2.23)
No learn (t-2)	-	0.125 (1.41)
No job (t-2)	-	-
High learn (t-3)	-	0.167 (3.03)
Moderate learn (t-3)	-	0.036 (0.57)
Low learn (t-3)	-	-0.015 (0.22)
No learn (t-3)	-	0.030 (0.40)
No job (t-3)	-	-
R^2	0.28	0.29
Sample size	392	444

[a] Civilian males not enrolled in school in 1979, last enrolled in 1978 or 1979 (prior to 1979 survey). For variable definitions see text. Absolute values of t-ratios are reported in parentheses.

it will be useful to review briefly the estimated effects of the other independent variables. Controlling for the various skill and experience factors, race and ethnic characteristics appear to have no significant impact on wage rates. Among the aptitude measures, only MECHANICAL (the sum of the scores on the auto shop information and mechanical comprehension subtests) has a significant positive effect on wage rates. Somewhat anomalously, the SCIENCE score is estimated to have a significant negative impact on log wage rate. With the inclusion of the aptitude scores, the schooling years coefficient is not significant.

The presence of collective bargaining at the workplace has a large, positive effect on the wage rates of these young men, close to a 30 percent increase in wage levels. The collective bargaining effect, though quite large, does not explain the systematic increase in wage over the 1979 to 1982 period for this group (Table 7.1). The estimated collective bargaining effect on wage rates in 1979 is almost identical to the effect in 1982 (Table 7.5, column 1). Moreover, the percentage of working youth covered by collective bargaining increased only slightly over time: 20 percent were covered by collective bargaining in 1979, 25 percent in 1980, 24 percent in 1981, and 24 percent in 1982.

Job Experience Effects on Wage. The effect of prior employment experience on wage rates in 1982 estimated above (Table 7.5, column 2) is presented in more convenient form in Table 7.6. The importance of job quality in the development of marketable skills is evident. A high learning job is estimated to have on average (column 4) a 13.5 percent effect on subsequent wages (relative to no job at all). The effect falls more or less proportionately by job learning content, 9.3 percent for moderate learning, 6.1 percent for low learning, and 6.3 percent for no learning. It would

Table 7.6 The Percentage on 1982 Wage Rate of Prior Employment State[a]

	(1) 1979	(2) 1980	(3) 1981	(4) Average
With job				
High learn	16.7%	13.2%	10.6%	13.5%
Moderate learn	3.6	12.2	12.1	9.3
Low learn	-1.4	15.2	4.4	6.1
No learn	3.0	12.5	3.4	6.3
No job	--	--	--	--

[a] Civilian males out of school in 1979, last enrolled in school in 1978 or 1979 (prior to first survey), and with job in 1982. The full regression results are reported in Table 3.

[b] Base.

appear from the last result that holding a job of any kind has future value, even one with no apparent learning content, perhaps because of the work discipline instilled in the employee (Ellwood, 1982).

The importance of the experience effects can be illustrated by considering a hypothetical individual with wage rate capabilities in 1979 of $4.00 per hour (the average for this sample in 1979). Assume for convenience that, were he to secure no job in the interim, his wage rate capabilities in 1982 would remain $4.00 per hour, perhaps not an implausible assumption if one ignores inflation effects. What would be the wage capabilities of the more fortunate members of his cohort with identical initial capabilities? Those who secured jobs with little or no learning capabilities in each year would have earning capacities of $4.80 per hour, 20 percent higher. Those fortunate enough to find regular employment with high learning opportunities would on average be earning $5.85 per hour or wages 46 percent greater after three years.

Equalizing Differentials. It is, of course, possible to define the equalizing differential in wages required to compensate an individual for greater or less learning opportunities on the job if he is not employed. Among those with a job, however, it is an important question to ask the extent to which the market for on-the-job training adjusts current wages to reflect differential learning opportunities across jobs. The regression results suggest that the individuals who secure high learning jobs receive windfall gains. Controlling for aptitude, collective bargaining, etc., youth in high learning jobs do not appear to suffer wage penalties for the learning opportunities.

The coefficients from the 1979 and the 1982 regressions for current learning opportunities on the job are collected for convenience in Table

7.7. The estimates indicate that wage rates on high learning jobs are higher, not lower than those on no learning jobs, 2.4 percent higher in 1979, 14.2 percent higher in 1982. Neither of the estimated effects are statistically significant at customary levels. Nonetheless, the evidence is quite strong that the effects are not negative as one might expect in an efficiently operating market for on-the-job training opportunities. It would appear that individuals are unwilling or unable because of institutional restrictions to bid on high learning jobs. Of course, these results may simply reflect unobserved heterogeneity in the sample, the more productive among those of given schooling, aptitude scores, etc. secure the high learning jobs.

IV. THE ALLOCATION OF JOBS WITH HIGH LEARNING CONTENT

An important question in the market determination of income distributions is, who secures the high learning jobs? The most prominent human capital investment model, that of Yoram Ben-Porath (1967) treats on-the-job training as simply an extension of schooling activity into a period in which individuals no longer specialize completely in human investment. One might expect from such a model that the individuals who secure high learning jobs following the schooling period would be the same type of individuals who would otherwise most likely be in school. Individuals would choose high learning jobs just as they might choose complete specialization in learning in an earlier period. In particular, one might expect that the high learning jobs would go to the most able individuals, those with high aptitude scores, and those from wealthier families with presumably lower capital costs.

The on-the-job training decision may, for a number of reasons, be quite different in character from the schooling decision. Since earnings of even high learning content jobs are likely to be positive, financial

Table 7.7 The Percentage Effect on Wage Rate in 1979 and 1982 of Learning Content of Current Job[a]

	1979	1982
High learn	2.4%	14.2%
Moderate learn	4.9	5.1
Low learn	-5.8	3.6
No learn[b]	--	--

[a] Civilian males out of school in 1979, last enrolled in school 1978 or 1979 (prior to first survey). Full regression results are reported in Appendix Table 1.

[b] Base.

characteristics and therefore family background may be less important. Moreover, on-the-job training, since it is tied to employment, is not simply the purchase of learning activities as is schooling. As discussed earlier, various wage restrictions such as minimum wages and union contracts (or the threat of unionization) may and apparently do restrict the ability of employers to "charge" training opportunities through reduced wages. Perhaps more importantly, the employer may in part be investing in the worker's skills specific to this particular job. The employer's selection criteria may therefore differ in a systematic way from the employee's decision process. With firm specific training, for example, indicators of low job mobility such as marital status may become relatively important, family background much less important.

To explore the similarities and differences in the schooling and on-the-job training decisions, a logit analysis of the post-high school schooling decision of male youth in the youth sample was undertaken as was a logit analysis of the probability of securing a high-learning job among recent school leavers. The logit structure can most easily be interpreted as an estimator of the log of the ratio of the probability of an event occurring to the probability that the event will not occur.[1]

Consider the facts likely to influence the probability that a high school senior, say in 1979, will be enrolled in school the following year, 1980. Typically these factors have included ability factors which may affect the productivity of schooling, family background which may affect the capital costs of schooling as well as taste or consumption elements of schooling (Parsons, 1975; Willis and Rosen, 1979). In Table 7.8, column 1, logit estimates of the post-high school enrollment decision are reported for those variables. The estimates for the males in the youth sample are consistent

Table 7.8 Logit Estimates of the Determinants of School Enrollment and of Job Experience among Recent School Leavers, Males, 1979[a]

	1980 Enrollment Probability Beyond Grade 12[b] (1)	Not Enrolled in 1979 Probability of: High-Learning Job 1979[c] (1)	Any Job 1979 (2)
INTERCEPT	-3.088	-4.442	-5.746
	(5.21)	(3.19)	(4.20)
DEMOGRAPHIC			
Black	1.103	-0.452	-0.773
	(3.39)	(1.51)	(3.00)
Hispanic	0.180	0.047	-0.017
	(0.20)	(0.10)	(0.00)
Age	--	0.128	0.250
		(1.19)	(2.43)
Schooling	--	0.097	0.115
		(0.89)	(1.20)
ASVAB			
Math	0.209	-0.028	-0.117
	(6.32)	(0.97)	(3.73)
Verbal	0.068	-0.011	0.012
	(1.55)	(0.30)	(0.32)
Science	0.080	-0.067	0.012
	(1.30)	(1.24)	(0.22)
Numerical	-0.008	0.015	0.037
	(0.56)	(1.23)	(2.92)
Mechanical	-0.178	0.0768	0.046
	(4.19)	(2.14)	(1.28)
FAMILY			
Father's schooling	0.110	0.048	0.045
	(2.58)	(1.41)	(1.35)
Mother's schooling	-0.054	-0.059	-0.000
	(0.52)	(1.40)	(0.00)
Chi Square	145.36	40.89	102.48
Observations	427	591	616

Table 7.8 (continued)

[a] Source: Youth sample of National Longitudinal Surveys. Absolute values of t-ratios are reported in parentheses. All schooling variables are measured in years. Not reported are dummy variables for "don't know" responses to schooling attainment of father or mother.

[b] Dummy equal to one if enrolled in school at time of 1980 survey, zero otherwise. Sample is males enrolled in the 12th grade at time of 1979 survey.

[c] Dummy equal to one if respondent believes statement that his current job has high learning potential is "very true", zero otherwise. Sample males not enrolled in school at time of 1979 survey but last enrolled in either 1978 or 1979.

with estimates for other data sets, (Willis and Rosen, 1979). The mathematics score on ASVAB is a powerful positive indicator of post-high school education. Family background, especially the level of schooling of the respondent's father, has a significant, independent effect on the schooling decision. Controlling for these factors, blacks are more likely than whites to continue their schooling. Finally, a result consistent with the Willis-Rosen comparative advantage argument appears, namely the negative and significant estimated coefficient of mechanical ability. Presumably this reflects the relative value of mechanical skills in the job market for this age/schooling group.

In column 2 of Table 7.8, the logit estimates of a similar model are reported for the factors that influence the probability that a recent school leaver will be holding a job with high learning content at the time of the 1979 survey interview. Since the sample is of all school leavers and not limited to high school seniors, controls for age and schooling attained are introduced into the model. The coefficient estimates of the on-the-job training model are quite unlike those in the schooling model. Mathematics aptitude scores are not only insignificant, they are a negative factor in the probability of holding a high learning job. Conversely, mechanical abilities has a significant positive effect on securing a high-learning job. The estimated coefficient of BLACK is negative, suggesting that, quite unlike schooling, blacks are less likely to be in high learning jobs when ability and family background characteristics are held constant, although the coefficient is not statistically significant at the two-tail 0.05 level. Father's schooling remains positive but much reduced in magnitude and statistical significance in the on-the-job learning model.

The pattern of coefficients in the high-learning model seems to suggest that the on-the-job learning process and the post-high school schooling decision are quite different. Indeed, the results are rather more consistent with a comparative advantage argument than with an on-the-job training as an extension of schooling argument. This conclusion is even clearer in logit estimates of the probability that a recent school leaver holds any job at the time of the 1979 survey. The coefficient estimates are quite close to the schooling enrollment estimates multiplied by -1. The likelihood that a black school leaver will hold any job is significantly negative, raising the possibility that blacks are disproportionately enrolled in school because of the difficulties they would otherwise have securing jobs as high school job leavers. Among the estimated ability coefficients, numerical computational ability (including coding speed) and mechanical ability have positive effects on job holding estimated in the immediate post-school period while conversely having negative effects on schooling enrollment. Abstract mathematical ability has a strong negative effect on job holding, quite the converse of its powerful positive role in schooling enrollment.

It is, of course, unlikely that abstract mathematical ability actually reduces the likelihood of securing a job in the post-schooling period. The negative coefficient is more likely the consequence of selection bias across other unobservable traits: individuals with high abstract mathematics scores who choose not to continue their schooling at this level are likely to have motivational and social problems disproportionately. A more elaborate statistical analysis, not complete at this time, will permit this hypothesis to be explored more completely. The important general point to be made here, however, is that the holding of a high learning job appears to be the result of a quite different process than that of schooling continuation beyond the period of complete schooling specialization. Additional research on the allocation of high learning jobs (or jobs at all for that matter) in the early post-schooling period is essential, if the labor market behavior and earnings record of this group is to be well understood.

IV. THE INDIVIDUAL PERSISTENCE OF ON-THE-JOB TRAINING OVER THE EARLY POST-SCHOOLING YEARS

The log wage rate estimates indicate quite clearly that job experience and the quality of the job experience are both important in the development of marketable skills. Mincer's on-the-job training hypothesis for early wage growth is therefore strongly and explicitly confirmed in the experience of young school leavers. In the last section, evidence on which individuals are most likely to secure jobs with high learning content was developed. In this section I consider: (1) how average levels of job learning evolve over the first few post-schooling years, and perhaps more importantly, (2) how job learning opportunities are distributed over individuals across time. The implications for the distribution of adult earnings are quite different if the same individuals are found in the high learning jobs over time than if the jobs are held by different individuals across years.

With no direct measure of on-the-job training, Mincer (1974) was forced to impose ad hoc assumptions on the form of on-the-job training investment. Specifically he assumed a linearly declining level of on-the-job training in the post-schooling years, an assumption which justified the introduction of experience and experience squared in the log wage equation (at least during the early post-schooling years when investment is positive). What is the time profile of on-the-job training implied by the directly measured indicators.

The probability that an individual in the sample held a job and therefore accumulated job experience of any type was stable over the period 1979 to

1982. See Table 7.9, Panel A. For the group as a whole, the employment ratio (the ratio of employment to population) was 70.5 percent in 1979, 71.4 percent in 1982. One might conjecture that the increase in employment one might normally expect with age did not occur because of the simultaneous increase in the severity of the recession. Schooling level increased the prospects for employment sharply. The probability of employment was 54.8 percent in 1979 for those with less than 12 years of schooling, 80.9 percent for those with 12 years, and 84.2 percent for those with more than 12 years. The schooling differentials remained quite stable over time.

More interesting is the average learning content of the job (Table 7.9, Panel B). The individuals in the sample perceived significant learning possibilities in all years. In 1979, for example, 37 percent felt their jobs offered high levels of learning opportunities, 64.5 percent high or moderate learning opportunities. Moreover, in contrast to the assumption of linearly declining on-the-job training in the post-schooling period, the individuals in this sample report increases in learning opportunities over time. By 1982, 46.5 percent indicate high learning opportunities, 80.1 percent high or moderate learning. The percentage who report no learning opportunities declines from 15.2 percent in 1979 to 6.1 percent in 1982. Far from declining, on-the-job training apparently increases in these early years as the individuals more successfully integrate themselves into the labor market. Although not reported in Table 7.6, differences in learning content of jobs (for those who secured jobs) did not vary substantially by schooling group.

Important for the distribution of annual earnings in subsequent years is the distribution of learning opportunities in the population over time. The data in Table 7.9 indicate that approximately 70 percent of the sample held

Table 7.9 The Post-School Employment Experience of Young Males, 1979-1982[a]

	1979	1980	1981	1982
	A. The Employment Ratio (in Percent)[b]			
Total schooling	70.5%	70.5%	69.0%	71.4%
S less than 12	54.8	55.6	50.0	57.1
S = 12	80.9	79.5	79.9	80.2
S greater than 12	84.2	86.8	92.1	86.8
	B. Learning Quality of Job (in Percent)[c]			
Total schooling only				
High learn	37.1%	42.9%	41.1%	46.5%
Moderate learn	27.4	29.2	33.4	33.6
Low learn	20.3	17.6	15.8	13.8
No learn	15.2	10.3	9.7	6.1
Total	100.0%	100.0%	100.0%	100.0%

[a] Civilian males not enrolled in 1979, last enrolled in 1978 or 1979 (prior to 1979 survey).

[b] The ratio of the number employed to the population. The sample is limited to those with a valid employment status response in all four years. The sample sizes by schooling level are: total (606), S less than 12 (252), S = 12 (278), and S greater than 12 (76).

[c] The sample sizes by year are: 1979 (509), 1980 (480), 1981 (488), and 1982 (512).

jobs of some sort in a given year and that, of these, 40 percent judged the job experience as highly valuable in developing marketable skills. The distributional consequences on future earnings are quite different if those who hold high learning jobs in one year are different from those who hold high learning jobs in another year. To what extent are such opportunities shared over time. The distribution of income will have a greater dispersion if the individuals in the sample are stable in their job holding and job learning characteristics across years, a lesser dispersion if the job and learning opportunities fluctuate randomly across individuals over time.

In Tables 7.10 and 7.11 the transition matrices across job and learning characteristics are reported for 1979-1980 and 1980-1981. The strong persistence over time in employment experience is obvious in both tables. In Table 7.9 (1979-1980), for example, more than one-half (53.1 percent) of those who reported no employment in 1979 did so in 1980 as well. Conversely, almost one-half (45.6 percent) of those who reported themselves at a job with high learning opportunities in 1979 reported a similar high learning experience in 1980 as well. Only 14.4 percent of those in high learning jobs in 1979 reported no current job at the time of the 1980 survey. These relationships appear to be quite stable; the transition matrix for 1980-1981 is similar in virtually all respects to the 1979-1980 matrix. Cumulative advantage is clearly a part of the labor market experience of the post-schooling group in this sample.

V. CONCLUSIONS

The analysis of the early post-schooling experience of a sample of males from the NLS youth cohort confirms the importance of job experience and on-the-job training in the evolution of wage rates over time. Individuals who in

Table 7.10 The Transition Matrix (in Percent) of Employment Status by Learning Opportunity, 1979-1980[a]

	Employment status 1980					
	No job	With job by learning				
Employment status 1979	(1)	(2) None	(3) Low	(4) Moderate	(5) High	Total
No job	53.1	6.1	5.6	13.4	21.8	100.0
With job by learning:						
None	30.5	22.0	13.6	18.6	15.3	100.0
Low	26.5	6.0	21.7	19.3	26.5	100.0
Moderate	25.9	3.4	12.9	29.3	28.4	99.9
High	14.4	5.6	12.5	21.9	45.6	100.0

[a] Civilian males, out of school in 1979, enrolled in 1978 or 1979 (prior to first interview). Sample size by 1979 employment status: no job (179), no learn (59), low learn (83), moderate learn (116), and high learn (160).

Table 7.11 The Transition Matrix (in Percent) of Employment Status by Learning Opportunity, 1980-1982

Employment status 1979	Employment status 1981 No job (1)	With job by learning (2) None	(3) Low	(4) Moderate	(5) High	Total
No job	60.3	4.3	4.3	11.4	19.6	99.9%
With job by learning:						
None	25.6	30.8	10.3	20.5	12.8	100.0
Low	25.0	6.9	23.6	25.0	19.4	99.9
Moderate	19.0	9.0	11.6	38.9	21.5	100.0
High	16.1	1.1	10.6	24.4	47.8	100.0

[a] Civilian males, out of school in 1979, enrolled in 1978 or 1979 (prior to first interview). Sample size by 1980 employment status: no job (184), job - no learn (39), low learn (72), moderate learn (121), and high learn (180.

any survey were strongly supportive of the statement that learning on the job was increasing their marketable job skills had subsequent wages 13.5 percent higher than individuals who had been previously with no work. Their subsequent wages were 7 percent higher than similar individuals who reported they were employed but that the job offered little or no learning opportunities.

The analysis does not indicate any compensating differentials in current wages for individuals in high learning jobs. Indeed, controlling for intellectual aptitude, previous on-the-job training, etc., those individuals in high learning jobs appeared to have higher, not lower wages although the estimates are not statistically significant from zero. A wide range of legal and institutional barriers to low wages, including legal minimums and trade union restrictions, may explain why the wages do not fall in high learning jobs which promise higher future wages. Apparently these higher future wages are not offset by lower current wages but accrue as pure economic rents to the fortunate holders of high learning jobs.

The market process by which these high learning jobs are allocated appears to be quite different than formal schooling. At the high school level, individual aptitudes for mechanical and numerical processes are relatively heavily demanded, but more abstract mathematics and verbal abilities are not. Family background characteristics such as parental schooling attainment are much less important in securing a high learning job than in continuing formal education at the college level, reflecting no doubt the reduced need for capital with on-the-job training. Indeed, if equalizing differentials, that is, low starting wages in high learning jobs, ceteris paribus, are restricted or eliminated by minimum wage restrictions, union contracts, etc., than those in high learning jobs require no special capital to maintain their consumption in the learning period.

The amount of on-the-job training that occurs in the early years is substantial. Forty percent of the sample report that it is "very true" that their job experience is valuable in developing their marketable skills. Moreover, the percentage of job holders who perceive significant learning in their jobs increases over time, at least over the first four years after school departure.

The experience of the individuals in the sample suggests that there is considerable persistence in on-the-job training experiences across time for the same individual. On a year-to-year basis approximately one-half of the individuals in a high learning job in one year report a high learning job at the next annual survey as well. Conversely, approximately one-half of those who report no job (and therefore no learning experience) at one survey date report no job in the next survey. Given the magnitude of the on-the-job training effect on wage rates and the apparent absence of equalizing differentials in current wages of jobs with high learning content, it would appear that on-the-job training contributes importantly not only to differences in future annual earnings across individuals, but to differences in human wealth across individuals.

Further analysis of this rich data set is clearly warranted if the private market provision of job training opportunities is to be well understood. Additional work on the attributes of the individual that makes him attractive to firms offering job training opportunities would be useful. Analysis of the "price elasticity" of the private market provision of high learning jobs would also be important. The estimation of minimum wage effects on job training imply that the supply of on-the-job training by firms is quite sensitive to the net costs of the training (Fleisher, 1981; Leighton and Mincer, 1981; Hashimoto, 1982). With one exception (a portion of the Leighton and Mincer study), however, all these studies have relied on indirect measures of training, particularly a "flattening" of the lifetime wage profile. A more careful analysis of this process using the direct measure available in the NLS should provide insight into the basic processes that generate private training opportunities in the U.S. economy as well as providing a direct contribution to understanding minimum wage effects on the job training of young workers.

Chapter 7

Notes

[1] Formally,

$$\ln \frac{\theta}{1-\theta} = X\beta$$

where θ is the probability of the event and X is a vector of factors that make the event more or less likely (reflected in the estimated values of the coefficient vector β).

Chapter 7

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Chapter 8

Mobility and Wages

by

Gilbert Nestel

I. INTRODUCTION

Social scientists and government policy makers continue to study the high rates of nonemployment among teenage youth, particularly among nonwhite teenagers, and the role that the federal government can play in reducing the economic and noneconomic consequences of their joblessness.[1] The loss of job opportunities is important because it comes at a time when these youth first enter the labor force as full-time participants with the intent of establishing a successful career path. It is also at these ages that youth seek firm-specific work experience and on-the-job training to augment their classroom education. A high degree of joblessness affects not only the availability and dating of these latter investments, but it also affects the choice between further full-time schooling or current full-time labor force participation.[2] Alternatively, persistent high rates of nonemployment, or anticipations of increasing levels of joblessness in the future, may discourage youth from seeking additional education, particularly if the expected economic return from these investments is low.

The introduction of the All-Volunteer Armed Forces (AVF) provided teenagers with another work alternative to civilian employment. Beginning in 1973, eligible males and females could voluntarily choose to work in the noncivilian sector of the economy. Black male teenagers, who historically have had higher levels of joblessness and lower civilian wages than their white male counterparts, have responded disproportionately to these opportunities.[3] The continued success of the AVF in the 1980s in attracting and retaining qualified youth of all races is explained by the less favorable job openings in the civilian sector for these youth, and the increasing competitiveness of the services both in pay and opportunities for training and education.

This paper has a more limited focus than to model the choices between school and employment opportunities in the civilian and noncivilian sectors. We abstract entirely from the school decision by restricting the universe to youth who left school permanently between 1976 and 1978. We also eliminate youth who completed less than 10 years or more than 14 years of schooling because the services select or attract very few enlistees from these educational categories. High school dropouts, for example, are generally unacceptable because they are significantly less likely than high school graduates to complete a first tour. The selection of the 1976-78 school leavers provides not only a sufficiently large sample of former AVF enlistees and youth who never served, but the post-school period is long enough to allow us to evaluate the transition between school and work. Our research strategy is to follow this sample of youth for five to seven years after they leave school, and to quantify the variations in their work and nonwork experiences as civilians and as participants in the armed forces. The different patterns of work behavior are then further differentiated by types of employers, kinds of work, and industries. The statistical analysis aims to assess the importance of sector of employment and work continuity, as well as changes in the characteristics of the jobs held, on the hourly earnings of those at work in civilian jobs between the 1982 and 1983 interview dates.

Youth with previous service in the AVF serve as an excellent comparison group to study these issues. The military is a significant employer of teenage youth.[4] Those who elect to serve are guaranteed year-round, full-time employment for the duration of time spent in the service. Periods of joblessness or weeks out of the labor force are not possible, and continuity of work is assured. In addition, the time spent in the military is generally in one occupation, with one employer, and in one industry. While occupational reassignments are possible, and a shift from one branch of service to another can occur, these changes are not very frequent, at least among enlistees in their first tour. The transition from school to the military is generally of a very short duration. Most youth who enlist commit themselves to a branch of service while in school by signing a contract that binds them to serve at a later date. Almost all new entrants in the Army, for example, are under a Delayed Entry Program (DEP), and better than 90 percent of first-term accessions in the other branches join through this program (Maze, 1985). Actual entry then takes place shortly after leaving school, usually within the same year that school ends. Although it is possible to leave the service before the completion of a tour, to do so requires the consent of the military authorities. Thus the decision to change employers is not solely at the discretion of the employee. A final distinguishing feature of the AVF is the pay schedule that a potential entrant faces. Although wages can vary in private sector jobs because of differences in local labor market conditions, in hiring practices of firms, and in the prior work experience and qualifications of the applicants, almost all first-term enlistees are assigned to the same pay grade at the time of entry. Compensation varies only because of differences in number of dependents, locational assignments, or enlistment bonuses that reward entrants who choose occupations that are in short supply.

In contrast, youth who seek civilian sector jobs often face periods of nonwork as they search for an entry position and some continuity of employment. These spells are not only present at the time of entry. Some weeks will be lost subsequently because of employer initiated lay-offs or firings. Even decisions to leave an employer voluntarily, either because of unsatisfactory work conditions or more favorable opportunities elsewhere, will be accompanied by periods of nonwork. To the extent that the new job is not "lined up" while at work, additional time will be needed to search for its replacement and to evaluate the alternatives. Variations in wages and differences in other conditions of employment further complicate the decision process. Thus we anticipate that the never-served youth in our sample will average fewer weeks of post-school work and more weeks of nonwork, but show a larger number of different kinds of work and employer changes than those who previously served in the AVF. The more limited mobility among those who serve, particularly while in the service, provides one extreme group to evaluate the economic returns to mobility.

This paper is organized as follows. Section II briefly summarizes the data base and the construction of the different mobility measures. Section III provides preliminary tabulations highlighting the transition period between school and work, and the association between the different mobility measures and wages. Section IV studies these relationships in a multivariate framework. The major conclusions and their implications are found in the concluding section.

II. THE DATA BASE

The National Longitudinal Survey of Youth Labor Market Experience (NLS) is a nationally representative sample of the 32.9 million civilian noninstitutional youth who were age 14-21 on January 1, 1979. The initial sample consisted of approximately 11.4 thousand respondents. Minority and low-income households were oversampled to make a sufficiently large number of sample cases available for socioeconomic statistical comparisons. An additional 1,280 youth in the same age range were selected from a list of men and women serving actively in the armed forces on September 30, 1978; in this group, again for statistical reasons, female enlistees were oversampled, so that they comprised about one-third of the military sample. In contrast to the nonmilitary sample, service members stationed outside the United States are included. Face-to-face reinterviews of this total cohort have been completed annually since 1979. Attrition from the sample has not been extensive: more than 95 percent of the original sample were reinterviewed in 1983.

The data collected in these surveys provide a detailed profile of each youth's labor market experiences. A history of all jobs held since January 1, 1978 is available, and for each job the employer, occupation, industry, hourly earnings, and beginning and ending dates, or the duration of employment, is known.[5] Periods of nonwork while on the job and between jobs are also available. Additional information in more aggregate form summarizes some of the youth's experiences prior to 1978. In particular, the annual weeks of work (nonwork) in each of the calendar years from 1975 to 1977, is known. As was the case for the post-1978 period, the school and nonschool experiences in this earlier period can be distinguished.

A number of post-school mobility measures have been developed from these extensive work histories to characterize various subgroups of out-of-school youth along different dimensions of their labor market activities, and to provide a test of the effect of their behavior on their wages. First, the number of different jobs held by these youth is calculated. We are also able to distinguish between jobs that satisfy the criteria of regular employment and those that are of shorter duration and/or work intensity.[6] Both sets of employer counts are further adjusted for periods of employment that are with the same employer in different years. While the work histories are not sufficiently detailed to differentiate between voluntary and involuntary separations, youth can be classified by the number of times they changed employers. Issues such as whether excessive turnover has a negative effect on wages can then be studied empirically. Second, we count the different kinds of work in the jobs held to ascertain whether changes in job assignments are rewarded financially. Growth in wages generally accompanies promotions or other changes in occupational assignment that reflect greater responsibilities on the job, or increases in status. Third, the number of industrial affiliations is counted to test whether youth who change their industry of employment more frequently are compensated differently. Do youth who move in response to perceived differences in growth potential of industries enjoy higher than average increments in pay? The occupation and industry counts are restricted to regular jobs only, but they are also adjusted for changes in the work done and the industrial setting. The periods of work (nonwork) for former AVF enlistees are also studied in detail.[7] Two measures are considered: the first assigns the time spent in the military to civilian work experience while the other includes these weeks among the weeks of nonwork. The research issue is the civilian pay of former enlistees and how employers in the private sector value work while in the military.

III. TABULAR FINDINGS

The set of tables presented in the Appendix and discussed in this section (and to some extent in the following one) summarize the relationships that are studied in greater detail in the multivariate analysis. These tables highlight the transition period between school and first job, and the extent and dimensions of their post-school work experiences. In each of these tables we control for whether or not the youth had ever served in the AVF. The universe is further stratified by race and sex to study differences in outcomes among different subgroups. The individual responses are weighted by the inverse of the sample weights, and the resultant estimate approximates the population parameter.

As indicated earlier, the transition period from school to work is considerably shorter for youth who choose to serve in the AVF (Table 8A.1). Overall, about 39 weeks elapsed before the average respondent found a regular job. Youth who never served averaged an additional one and one-half weeks to find employment. In contrast, the average AVF enlistee became active within 12 weeks of leaving school; moreover, the transition period did not vary by the enlistee's subsequent behavior in the military such as completion status in first tour or reenlistment.

Women who never served in the AVF averaged about a year before they located their first regular job--a period that was about two times longer in duration than the average time reported by comparably-aged men. The average female youth who subsequently joined the AVF also delayed her entry for an additional five and one-half weeks beyond the time the average male enlistee experienced. Racial differences in behavior were also present among the men. Whites were more likely than blacks in both sectors of employment to make the transition between school and regular work in a shorter time span. Among the never-served, for example, the corresponding averages were about 43 weeks for blacks and about 22 weeks for whites. A further stratification of the sample by the year they left school did not alter the direction of the differences, even though some of the magnitudes varied by reference year.

One of the more effective ways youth can ease their transition into the world of work is to continue working in the same job held while enrolled in school. The likelihood of this happening is not insignificant. About three in 10 youth who left school in 1978 began their first post-school regular job while in school (Table 8.A2). Men were about six percentage points more likely than women to have this happen. In contrast, women were considerably more likely than men to spend longer periods of time selecting a regular job--a finding that is consistent with their higher mean duration of search time between school and work that was reported earlier. Again focusing on the 1978 school leavers, we find that almost eight percent of the women but less than four percent of the men required as long as two to five years before deciding on a regular job.

The combination of a higher likelihood that the first job will be a continuation of a school job and a lower likelihood of a long search period suggests that men will spend more of their post-school exposure period at work than women. We also expected that former enlistees will average more weeks of work than the never-served if the period in the military is counted as work experience. The latter assumption also suggests that the weeks of work for veterans would exceed the work experience of attriters since the former spend a considerably longer period of time in the military than the latter.

These conjectures are supported by the data (Table 8A.3, upper panel). The sample of all youth and the never-served subgroup averaged about 209 weeks of work. Veterans reported an additional 43 weeks above the average, while attriters worked about nine weeks below the average. Women averaged about 42 fewer weeks of work than men, and black men averaged 38 weeks less work than white men. Veterans consistently had the longest commitment to work while attriters were generally the least attached, particularly among the women.

Female attriters averaged about 72 fewer weeks of work than female veterans and about 59 fewer weeks than the never-served.

As expected, the rankings change dramatically when the military experience is excluded from civilian work and assigned to weeks of nonwork. The veteran is now at the bottom of the rankings with the never-served showing the largest number of weeks of work (Table 8A.3, lower panel).

Tables 8A.4-8A.6 summarize the number of post-school civilian jobs held and selected characteristics of these jobs.[8] The periods of employment in these jobs are initially differentiated by duration and hours of work, and then further classified by whether or not they were with the same employer. Occupational assignments and industrial attachments extend to all post-school regular jobs, not only to jobs held in adjacent years, as is the case with the counts of unique employers.

The mobility counts are consistent with our earlier rankings of subgroups by the magnitude of their civilian work experiences. This association is expected since the opportunities or need to change employers and job assignments should vary with duration of employment.[9] Veterans who averaged the smallest number of civilian weeks of work also had the lowest employer turnover counts and were the most likely of the subgroups to be in the same industry and type of work.[10] Female veterans, in particular, were the least mobile subgroup; they averaged about two changes of employers, occupations, and industries. At the other extreme of the continuum were the never-served white males, the most mobile group, who changed employer, occupation, and industry between three and four different times, depending on the definition used.

The economic consequences of the different patterns of mobility are measured by the youth's wage in 1983, and the association between these two measures by type of movement is found in Tables 8A.7-8A.9. Our interest in these tables is twofold: first, we document the nonlinear association between each of the mobility measures and hourly earnings; and second, we explore the differences in earnings between workers who remain with one employer, work in one occupation, or identify with one industry, and those with work histories that are more variable. To simplify the exposition, we focus on the unique experiences in regular jobs only; as a consequence, periods of employment with the same employer in different years, or similar kinds of work with different employers, are excluded.

We expected nonlinear associations for obvious reasons. As indicated earlier, some employee separations are involuntary and initiated by employers through lay-offs or firings. Subsequent reemployment with a new employer, even a change in job title or responsibility, or a movement to a different industry, may not be accompanied by an increase in hourly earnings. The conditions of the local labor market, the extent of the search activities by the worker, and how employers value prior work experience dictate in large part whether the worker will be at a financial disadvantage in the new job.

Voluntary separations are more likely to result in wage increases, but these gains may disappear as the number of such moves increases. Employers may value the initiative and prior work experiences of a relatively long-tenured worker who is seeking to improve his/her economic position; however, the same employer may have strong reservations about a worker whose work history indicated very short attachments with prior employers. At issue here is the relationship between turnover, worker characteristics, and productivity; unfortunately, we have no way of objectively quantifying the latter concept.

The economic returns to job-specific skills and the costs of training new personnel suggest that firms will try to retain their most productive workers. Employees, in turn, generally acquire rights to pensions, seniority, and promotion only with increases in tenure. Thus we would expect youth who remain with a single employer for longer periods of time to have above-average wages. Whether these compensations exceed the wages of the more mobile workers remains an empirical question. In any event, the higher pay expected among longer-tenured employees is another reason why the association between wages and employer mobility should be nonlinear.

The data support these arguments, but not very consistently. Youth who worked with only one employer generally earned the highest average hourly pay (Table 8A.7, lowest panel). Additional increases in the number of employer changes were generally accompanied by a decline in hourly compensation (despite an occasional upturn), at least among the men. The same pattern emerged for the women, but the association was even more erratic.

The expected relationship between wages and changes in occupation is even more uncertain. Promotions are often accompanied by changes in job titles, new responsibilities, and increases in pay. Pay increments based solely on seniority with no change in job assignment are not unusual occurrances, however (e.g., school teachers, military personnel). Thus we are not very surprised to find that workers who remained in the same kind of work did not receive the highest hourly earnings (Table 8A.8, lower panel). Those who changed occupations very frequently were also at some pay disadvantage.

The highest degree of uncertainty surrounds the industry-wage association. Employer changes with no change in occupations or industry are not uncommon (e.g., construction), but the expected direction of the wage change is not obvious, for the reasons indicated earlier. A change in industry and employer but no change in job assignment often leads to positive wage gains. Other combinations result in different consequences. The empirical evidence for this age group of workers showed almost no association between wages and industrial changes (Table 8.A9, lower panel).

IV. MULTIVARIATE ANALYSIS

Introduction

Thus far the statistical analysis has concentrated on the different mobility counts and their association with wages for various subgroups in the population and by different sectors of employment. We now turn from this earlier tabular analysis to a multivariate appraisal that allows for a more complete control and assessment of other factors expected to be correlated with mobility measures, work experience, and wages. The same demographic subgroups identified in the tabular presentation continue to be studied separately because of the known interaction (nonlinear) effects of race and sex on hourly compensation. Youth who previously served in the AVF are singled out as the reference group. Tabular estimates continue to incorporate the different sample weights of the respondents, but the multivariate estimates are based on unweighted observations; i.e., each respondent is assigned the same unitary weight.

It is instructive at the outset to profile the sample of youth by their labor force and employment status in survey week 1983, and to identify the number who were not at work in that week so that the potential bias because of their omission is known. We also highlight their 1983 wage levels by sector of employment to establish which groups were at a pay disadvantage (advantage), and then assess whether these differences can be explained by variations in achievement levels. The wages of these workers in their first post-school jobs are also contrasted to see whether the same differentials were present at that earlier date.

About one in six (17.7 percent) of the youth in the sample were not members of the labor force at the time of interview in 1983 (Table 8A.10). Females were about three and one-half times more likely than males to be in this category, and among the males, blacks were about one and one-half times more likely than whites. Attriters were consistently more likely than veterans to be out of the labor force, with female attriters the most likely of the groups (one in two). Former enlistees were also more likely than the never-served group to the unemployed, and with the exception of the black male subgroup, less likely to be employed. At the extreme, about four in five never-served white males but only about four in 10 female veterans had a job in survey week 1983.

The variations in wages among youth with work experience in the 1982-83 period is summarized in Table 8A.11. Overall, working youth earned $6.09 per hour, but this average varied from a low of $3.52 for female attriters to a high of $7.29 for white males who never served. The hourly earnings of former enlistees were generally below the average for the never-served except among black males, where the two averages were very similar. Male attriters and veterans were compensated at about the same hourly rate, but female veterans averaged about two dollars an hour more than female attriters. It is also of interest to note that working women were at a considerable disadvantage in hourly pay compared to their male counterparts, and that this difference was also present for men and women who previously served. Women were compensated at 72 percent of the hourly rate received by men in the never-served category; the pay difference for those with previous service experience was somewhat larger, with women's hourly earnings about 69 percent of what men averaged. The latter difference is explained by the substantial discrepancy in compensation between men and women who did not complete their first tour (attriter); the sex difference in pay disappeared to a large extent when the comparison was restricted to veterans of each sex.

Differences in level of achievement, as measured by AFQT scores, do not explain why former enlistees were less attached to the workforce or compensated at a lower hourly rate when at work. In the aggregate, in each of the three school-leaving years, and in each subgroup, the ever-served score exceeded the average for the never-served (Table 8A.12). Female enlistee scores generally exceeded their male counterparts, and the female attriters were on average the most qualified group. Black male scores averaged two-thirds the magnitude of the white male scores, but here again the scores for former black enlistees exceeded the average for blacks who never served.

The pay disadvantage that the average youth with prior military experience faced in 1982-83 was not evident at the time of enlistment. Youth who chose noncivilian employment as their first post-school regular job averaged at least as high an hourly compensation as his counterparts who chose civilian sector jobs (Table 8A.13). Moreover, enlistees who worked at a regular civilian job prior to entry in the AVF were the most advantaged pay group. Women who joined also improved their economic position and were compensated at a higher rate than their never-served counterparts; hourly pay premiums in favor of the former group averaged between 15 and 25 percent depending on the year the first post-school job began. It needs also to be recalled that women and men of all races are compensated, with few exceptions, at the same hourly rate at the time of their entry into the AVF. Thus race and sex differences in pay in entry level jobs in the private sector do not generalize to military employment.

Specification

The (ln) wage specification includes controls for race (HISPANIC and BLACK) when the samples of males and females are analyzed (WHITE is the reference group), and human capital measures such as years of regular school completed (EDUCATION), achievement (AFQT score), and health (HEALTH) that are known to affect both work experience, mobility and wages.[11] Place of residence characteristics such as region (SOUTH) and size (SMSA) are also included to control for differences in wages because of variations in cost of living and job opportunities. The inclusion of a collective bargaining measure (UNION) recognizes its importance at the work site for wages, nonwage benefits, and stability of employment. Separate dichotomous variables are included to distinguish between ATTRITER and NEVER SERVED (VETERAN is the reference group). The post-school work (WKS WORK) and nonwork (WKS NONWORK) variables measure the effect of work (nonwork) experience on wages as well as control for differences in post-school exposure periods. The experience measures are calculated in two ways, as we noted earlier, to reflect the different assumptions about how civilian employers value the military experience of former enlistees. In the first formulation, the weeks in the military are included among the weeks of civilian work, while the alternative approach assumes that these weeks are completely discounted and considered weeks of nonwork. The inclusion of TENURE recognizes that the duration of attachment to an employer is affected by earlier decisions about employer turnover as well as employer initiated layoffs or discharges, but it also measures the return to job-specific skills. The final set of variables, EMPLOYERS and OCCUPATIONS, control for the number of different employers and the number of different job assignments of the worker. Our earlier tabular analysis indicated a nonlinear association between these counts and wages; we introduce quadratic terms to test for this possibility. We retain the linear specification between work experience and wages because of the young ages of these workers and their recent entry into the work force. We also recognize the possibility of a specification error in the wage equation because of the correlation between the unexplained factors affecting the work decision and the factors affecting hourly pay. The estimation of an employment probability equation (PROBIT) and the subsequent introduction of LAMBDA in the wage equation attempts to control for this potential bias (Heckman, 1979). Our research strategy is to assume initially that the military experience is equivalent to civilian work and to then sequentially introduce two mobility measures as additional factors affecting wages. A final formulation assigns the military experiences to periods of nonwork and reestimates the wage equation (OLS) with the most general formulation of the mobility constructs. The same approach is repeated for a more restricted universe that only includes youth at work in survey week 1983.[12]

Findings

A number of generalizations emerged from the empirical findings summarized in Tables 8.1 to 8.4. The coefficients estimate the average change in wages (in percent) that accompany a (small) change in a specific characteristic after statistically controlling for the intercorrelations between the characteristic and all other factors included in the wage equation. Our discussion of these coefficients implies that the prior statistical adjustments have been made. The employment probability equation and its summary statistics, and the summary statistics for the different wage equations are found in Tables 8A.19 and 8.5, respectively.

Table 8.1 (ln) Wage Regressions, Male Youth: 1983[a]

(t-ratios in parentheses)

Characteristic	Specification 1	2	3	4	5
AFQT	0.0010	0.0009	0.0009	0.0009	0.0011
	(0.76)	(0.73)	(0.74)	(0.74)	(0.88)
EDUCATION	0.0146	0.0135	0.0129	0.0131	0.0132
	(0.76)	(0.69)	(0.66)	(0.67)	(0.68)
SOUTH	-0.0702	-0.0706	-0.0713	-0.0709	-0.0611
	(-1.39)	(-1.39)	(-1.40)**	(-1.39)*	(-1.20)
HISPANIC	0.0443	0.0456	0.0451	0.0458	0.0439
	(0.93)	(0.96)	(0.94)	(0.96)	(0.92)
BLACK	-0.0017	-0.0027	-0.0011	-0.0017	-0.0079
	(-0.03)	(-0.05)	(-0.02)	(-0.03)	(-0.13)
SMSA	0.0661	0.0664	0.0659	0.0659	0.0747
	(1.96)**	(1.96)**	(1.95)*	(1.94)*	(2.21)**
UNION	0.2937	0.2923	0.2920	0.2918	0.2878
	(8.67)**	(8.60)**	(8.59)**	(8.57)**	(8.48)**
NEVER SERVED	0.1133	0.1180	0.1219	0.1224	-0.2113
	(2.78)**	(2.85)**	(2.85)**	(2.84)**	(-3.34)**
ATTRITER	0.0578	0.0652	0.0669	0.0680	-0.1029
	(0.79)	(0.88)	(0.90)	(0.91)	(-1.30)
TENURE	0.0023	0.0023	0.0023	0.0022	0.0021
	(4.08)**	(3.81)**	(3.80)**	(3.78)**	(3.57)**
WKS WORK	0.0005	0.0004	0.0004	0.0004	
	(1.38)	(1.31)	(1.30)	(1.28)	
WKS NONWORK	-0.0013	-0.0013	-0.0014	-0.0014	
	(-3.25)**	(-3.27)**	(-3.28)**	(-3.28)**	
LAMBDA	-0.9958	-1.0075	-1.0123	-1.0087	-0.9432
	(-2.64)**	(-2.66)**	(-2.67)**	(-2.65)**	(-2.49)**
HEALTH	0.0695	0.0755	0.0770	0.0754	0.0661
	(0.52)	(0.56)	(0.57)	(0.56)	(0.49)
EMPLOYERS[b]		-0.0035	-0.0022	-0.0046	-0.0039
		(-0.64)	(-0.32)	(-0.30)	(-0.25)
OCCUPATIONS[c]			-0.0039	-0.0084	-0.0135
			(-0.36)	(-0.24)	(-0.39)
EMPLOY SQ				0.0002	0.0002
				(0.16)	(0.15)
OCC SQ				0.0006	0.0008
				(0.15)	(0.21)
WKS WORK (REV)[d]					0.0006
					(1.90)*
WKS NONWORK (REV)[e]					-0.0013
					(-3.26)**
INTERCEPT	5.9586	5.9955	6.0111	6.0223	6.2926
	(21.24)**	(20.93)**	(20.74)**	(20.34)**	(21.12)**
$\bar{R}^2$	0.238	0.238	0.237	0.235	0.242
Sample cases	887	887	887	887	887

Table 8.1 (continued)

[a] Male youth with civilian work experience as civilian wage and salary earners between 1982 and 1983 interview dates, and who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Number of different civilian jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[c] Number of different kinds of civilian work engaged in between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[d] Excludes weeks of military service for youth who previously served.

[e] Includes weeks of military service for youth who previously served.

* Significant at $\alpha = 0.05$, one-tail test.
**Significant at $\alpha = 0.025$, one-tail test.

Table 8.2 (ln) Wage Regressions, White Male Youth: 1983[a]

(t-ratios in parentheses)

Characteristic	Specification				
	1	2	3	4	5
AFQT	-0.0002 (-0.07)	-0.0003 (-0.09)	-0.0003 (-0.09)	-0.0003 (-0.09)	-0.0004 (-0.13)
EDUCATION	0.0302 (0.99)	0.0266 (0.88)	0.0265 (0.87)	0.0274 (0.90)	0.0275 (0.91)
SOUTH	-0.0042 (-0.06)	-0.0032 (-0.05)	-0.0035 (-0.05)	-0.0057 (-0.08)	-0.0048 (-0.07)
SMSA	0.0455 (1.13)	0.0491 (1.22)	0.0489 (1.21)	0.0474 (1.16)	0.0520 (1.29)
UNION	0.3060 (7.03)**	0.3027 (6.95)**	0.3027 (6.95)**	0.3022 (6.92)**	0.2996 (6.92**
NEVER SERVED	0.0833 (1.68)*	0.0974 (1.93)*	0.0984 (1.89)*	0.1023 (1.96)	-0.2004 (-2.39)**
ATTRITER	0.0228 (0.18)	0.0428 (0.34)	0.0436 (0.34)	0.0450 (0.35)	-0.1013 (-0.77)
TENURE	0.0032 (4.50)**	0.0030 (4.07)**	0.0030 (4.07)**	0.0029 (4.04)**	0.0027 (3.68)**
WKS WORK	0.0003 (0.80)	0.0003 (0.67)	0.0003 (0.67)	0.0003 (0.71)	
WKS NONWORK	-0.0010 (-1.92)*	-0.0010 (-1.87)**	-0.0010 (-1.87)*	-0.0010 (-1.85)*	
LAMBDA	-1.1836 (-1.32)	-1.1937 (-1.33)	-1.1959 (-1.33)	-1.1927 (-1.32)	-1.2129 (-1.36)
HEALTH	0.1205 (0.61)	0.1371 (0.69)	0.1378 (0.69)	0.1484 (0.74)	0.1607 (0.81)
EMPLOYERS[b]		-0.0100 (-1.47)	-0.0096 (-1.15)	-0.0028 (-0.15)	0.0015 (0.08)
OCCUPATIONS[c]			-0.0012 (-0.09)	-0.0422 (-0.94)	-0.0458 (-1.03)
EMPLOY SQ				-0.0006 (-0.49)	-0.0007 (-0.62)
OCC SQ				0.0048 (0.94)	0.0046 (0.91)
WKS WORK (REV)[d]					0.0006 (1.37)
WKS NONWORK (REV)[e]					-0.0012 (-2.41)**
INTERCEPT	5.8793 (17.48)**	5.9760 (17.45)**	5.9811 (17.23)**	6.0226 (17.09)**	6.2911 (17.73)**
$\bar{R}^2$	0.195	0.197	0.195	0.194	0.207
Sample cases	587	587	587	587	587

Table 8.2 (continued)

[a] White male youth with civilian work experience as wage and salary earners between 1982 and 1983 interview dates, and who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Number of different civilian jobs held between the date left school and the date of interview in 1983. Excludes all work experience prior to January 1, 1978.

[c] Number of different kinds of civilian work in regular jobs held between the date left school and the date of interview in 1983. Excludes all work experience prior to January 1, 1978.

[d] Excludes weeks of military service for youth who previously served.

[e] Includes weeks of military service for youth who previously served.

* Significant at $\alpha = 0.05$, one-tail test.
**Significant at $\alpha = 0.025$, one-tail test.

Table 8.3 (ln) Wage Regressions, Black Male Youth: 1983[a]

(t-ratios in parentheses)

Characteristic	Specification				
	1	2	3	4	5
AFQT	0.0034	0.0033	0.0035	0.0034	0.0041
	(1.55)	(1.53)	(1.59)	(1.55)	(1.86)*
EDUCATION	-0.0056	-0.0002	-0.0028	-0.0003	0.0015
	(-0.14)	(-0.01)	(-0.07)	(-0.01)	(0.04)
SOUTH	-0.1704	-0.1686	-0.1667	-0.1494	-0.0970
	(-1.35)	(-1.33)	(-1.32)	(-1.17)	(-0.75)
SMSA	0.0725	0.0797	0.0812	0.0973	0.1401
	(0.77)	(0.84)	(0.86)	(1.02)	(1.45)
UNION	0.1715	0.1852	0.1819	0.1854	0.1784
	(2.54)**	(2.72)**	(2.66)**	(2.71)**	(2.57)**
NEVER SERVED	0.1925	0.1663	0.1860	0.1689	-0.3276
	(1.99)**	(1.69)*	(1.84)*	(1.65)*	(-2.59)**
ATTRITER	0.1738	0.1487	0.1421	0.1471	-0.0732
	(1.30)	(1.11)	(1.06)	(1.09)	(-0.51)
TENURE	0.0006	0.0011	0.0011	0.0011	0.0014
	(0.43)	(0.83)	(0.80)	(0.79)	(1.05)
WKS WORK	0.0014	0.0014	0.0014	0.0014	
	(1.80)*	(1.84)*	(1.74)*	(1.82)*	
WKS NONWORK	-0.0018	-0.0017	-0.0019	-0.0018	
	(-2.11)**	(-1.98)**	(-2.12)**	(-1.96)**	
LAMBDA	-0.7456	-0.7057	-0.6923	-0.6507	-0.4662
	(-1.68)*	(-1.58)	(-1.55)	(-1.46)	(-1.03)
HEALTH	-0.0736	-0.0995	-0.1061	-0.1166	-0.2005
	(-0.27)	(-0.36)	(-0.39)	(-0.43)	(-0.73)
EMPLOYERS[b]		0.0175	0.0257	-0.0213	-0.0277
		(1.28)	(1.53)	(-0.47)	(-0.60)
OCCUPATIONS[c]			-0.0195	0.0702	0.0576
			(-0.83)	(1.01)	(0.82)
EMPLOY SQ				0.0045	0.0050
				(1.15)	(1.28)
OCC SQ				-0.0095	-0.0083
				(-1.34)	(-1.15)
WKS WORK (REV)[d]					0.0017
					(2.06)**
WKS NONWORK (REV)[e]					-0.0012
					(-1.40)
INTERCEPT	5.9314	5.7680	5.8450	5.6861	5.9588
	(8.78)**	(8.41)**	(8.44)**	(8.03)**	(8.29)**
$\bar{R}^2$	0.306	0.308	0.307	0.308	0.296
Sample cases	194	194	194	194	194

Table 8.3 (continued)

[a] Black male youth with civilian work experience as wage and salary earners between 1982 and 1983 interview dates, and who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Number of different civilian jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[c] Number of different kinds of civilian work in regular jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

Excludes weeks of military service for youth who previously served.

[e] Includes weeks of military service for youth who previously served.

* Significant at $\alpha = 0.05$, one-tail test.
**Significant at $\alpha = 0.025$, one-tail test.

Table 8.4 (ln) Wage Regressions, Female Youth: 1983[a]

(t-ratios in parentheses)

Characteristic	Specification				
	1	2	3	4	5
AFQT	0.0076	0.0076	0.0076	0.0076	0.0076
	(4.99)**	(4.96)	(4.98)**	(5.01)**	(4.97)**
EDUCATION	0.0652	0.0639	0.0617	0.0609	0.0613
	(2.11)**	(2.06)**	(1.99)**	(1.96)**	(1.97)**
SOUTH	0.0019	0.0011	0.0037	0.0050	0.0027
	(0.05)	(0.03)	(0.10)	(0.13)	(0.07)
HISPANIC	0.1094	0.1084	0.1088	0.1093	0.1069
	(2.33)**	(2.31)**	(2.32)**	(2.33)**	(2.28)**
BLACK	0.1191	0.1182	0.1209	0.1179	0.1150
	(2.49)**	(2.47)**	(2.52)**	(2.45)**	(2.39)**
SMSA	0.1136	0.1143	0.1160	0.1179	0.1207
	(3.08)**	(3.09)**	(3.14)**	(3.18)**	(3.26)**
UNION	0.1505	0.1500	0.1487	0.1502	0.1513
	(3.74)**	(3.72)**	(3.69)**	(3.72)**	(3.75)**
NEVER SERVED	0.2314	0.2347	0.2576	0.2560	-0.0226
	(3.33)**	(3.36)**	(3.62)**	(3.58)**	(-0.28)**
ATTRITER	0.0773	0.0786	0.0957	0.0947	0.0478
	(0.87)	(0.89)	(1.07)	(1.06)	(-0.53)
TENURE	0.0016	0.0015	0.0015	0.0015	0.0015
	(2.70)**	(2.51)**	(2.47)**	(2.43)**	(2.41)**
WKS WORK	0.0006	0.0006	0.0006	0.0006	
	(1.74)*	(1.70)*	(1.77)*	(1.76)*	
WKS NONWORK	-0.0010	-0.0010	-0.0010	-0.0010	
	(-2.49)**	(-2.51)**	(-2.66)**	(-2.61)**	
LAMBDA	0.4287	0.4184	0.4095	0.4113	0.4067
	(2.14)**	(2.07)**	(2.03)**	(2.03)**	(2.01)**
HEALTH	-0.3087	-0.3048	-0.3040	-0.3074	-0.3064
	(-4.05)**	(-3.96)**	(-3.96)**	(-3.99)**	(-3.98)**
EMPLOYERS[b]		-0.0025	0.0040	-0.0116	-0.0117
		(-0.41)	(0.56)	(-0.67)	(-0.67)
OCCUPATIONS[c]			-0.0185	0.0004	0.0005
			(-1.69)*	(0.01)	(0.01)
EMPLOY SQ				0.0013	0.0013
				(1.00)	(1.00)
OCC SQ				-0.0021	-0.0021
				(-0.48)	(-0.47)
WKS WORK (REV)[d]					0.0007
					(1.93)*
WKS NONWORK (REV)[e]					-0.0009
					(-2.45)**
INTERCEPT	4.2084	4.2421	4.2883	4.2919	4.5512
	(7.90)**	(7.86)**	(7.95)**	(7.90)**	(8.35)**
$\bar{R}^2$	0.176	0.175	0.177	0.176	0.177
Sample cases	773	773	773	773	773

Table 8.4 (continued)

[a] Female youth with civilian work experience as wage and salary earners between 1982 and 1983 interview dates, and who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Number of different civilian jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[c] Number of different kinds of civilian work in regular jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[d] Excludes weeks of military service for youth who previously served.

[e] Includes weeks of military service for youth who previously served.

* Significant at $\alpha = 0.05$, one-tail test.
**Significant at $\alpha = 0.025$, one-tail test.

Table 8.5 Means and Standard Deviations for (Ln) Wage Equations, by Sex and Race: 1983[a]

(standard deviations in parentheses)

Characteristics	Male White	Male Black	Male Total	Female
AFQT	74.21	48.51	67.11	69.50
	(16.70)	(17.87)	(20.38)	(17.69)
EDUCATION	11.99	11.90	11.96	12.21
	(0.77)	(0.79)	(0.80)	(0.79)
SOUTH	0.28	0.59	0.35	0.38
	(0.45)	(0.49)	(0.48)	(0.49)
HISPANIC			0.12	0.13
			(0.32)	(0.33)
BLACK			0.22	0.21
			(0.41)	(0.41)
SMSA	0.70	0.75	0.72	0.72
	(0.46)	(0.44)	(0.45)	(0.45)
UNION	0.23	0.32	0.26	0.15
	(0.42)	(0.47)	(0.44)	(0.36)
NEVER SERVED	0.73	0.77	0.75	0.89
	(0.45)	(0.42)	(0.43)	(0.31)
ATTRITER	0.07	0.09	0.07	0.05
	(0.26)	(0.29)	(0.26)	(0.22)
TENURE	45.46	42.27	44.61	46.06
	(27.79)	(26.19)	(27.35)	(26.68)
WKS WORKED	243.56	220.61	236.50	215.33
	(61.32)	(69.02)	(64.69)	(69.19)
WKS NONWORK	44.39	65.76	50.81	68.14
	(46.40)	(59.10)	(51.49)	(61.51)
LAMBDA	0.12	0.26	0.15	0.33
	(0.07)	(0.16)	(0.10)	(0.19)
HEALTH	0.02	0.03	0.02	0.06
	(0.15)	(0.16)	(0.15)	(0.23)
EMPLOYERS[b]	4.09	3.77	4.10	3.76
	(2.87)	(2.48)	(2.83)	(2.55)
OCCUPATIONS[c]	3.88	3.93	3.90	3.62
	(1.77)	(1.79)	(1.75)	(1.66)
EMPLOYER SQ	24.94	20.34	24.85	20.60
	(41.98)	(27.60)	(40.26)	(31.24)
OCC SQ	18.13	18.61	18.29	15.86
	(15.54)	(16.92)	(15.68)	(13.65)
WKS WORKED (REV)[d]	200.24	188.05	198.46	201.07
	(86.82)	(81.48)	(84.63)	(79.02)
WKS NONWORK (REV)[e]	87.71	98.32	88.85	82.39
	(87.82)	(80.89)	(84.94)	(74.19)
WAGE (cents)	687.67	571.67	664.21	511.26
	(333.28)	(306.86)	(328.33)	(205.56)
(LN) WAGE	6.42	6.22	6.38	6.15
	(0.49)	(0.50)	(0.49)	(0.44)
Sample cases	587	194	887	773

Table 8.5 (continued)

[a] Youth with civilian work experience as wage and salary earners between 1982 and 1983 interview dates, and who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Number of different civilian jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[c] Number of different kinds of civilian work in regular jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[d] Excludes weeks of military service for youth who previously served.

[e] Includes weeks of military service for youth who previously served.

Wages of male youth five to seven years after leaving school were not altered significantly by differences in educational attainment, achievement levels, or changes in health status, but these human capital measures did affect the wage levels of female workers.[13] Moreover, neither the significance nor the magnitude of these responses depended on the specification of the mobility measures or on the assumption about how employers valued the military experiences of former enlistees. Working women translated each additional year of school completed to about a six to seven percentage point increase in wages, which was the same average increase in hourly compensation attributable to each 10-point increment in AFQT score. The health status of these women also affected their wages. Women with a health problem that limited their choice of job assignment or work effort earned on average about 35 percent less each hour than women with no reported work limitations because of their health.[14]

Characteristics associated with place of residence affected the compensation of young men and women differently. Female workers, for example, earned about 12 percent more per hour if they lived in an SMSA, but the hourly earnings of working men were unrelated to the size of their community. Region of residence had no independent effect on wage levels for either sex.

White and black males received about the same hourly pay, ceteris paribus; in contrast, Hispanic and black females averaged about 11 percent more in hourly earnings than white females. Thus, the "unadjusted" pay advantage that white male workers enjoyed over their black counterparts (about $1.50 per hour) disappears when factors correlated with race and pay are included.

The presence of a collective bargaining unit to represent workers in wage negotiations had a positive effect on the level of the compensation. Men and women in the aggregate, as well as white and black men, had higher average wage levels when represented than when forced to negotiate alone. These returns also varied by subgroup, with white males profiting the most (about 35 percent) and females the least (16 percent). Here again the significance and magnitude of the net relationship between union representation and wages was unaffected by alternative specifications of the wage equation.

Seniority on the job generally had a positive effect on wages, but the magnitude of the wage gain varied by subgroup. At one extreme, black males' wages were found to be unrelated to the number of weeks of work with their 1983 survey week (last) employer. At the other extreme, white males were able to convert each 10 week period of attachment to an employer to an average wage gain of about three percent. The corresponding gain for female workers was about one half this magnitude, or about 1.5 percent for each 10 weeks worked with an employer.

The data provide conflicting evidence that wages were positively influenced by longer durations of civilian work and negatively influenced by nonwork periods, and that veterans were compensated at a higher hourly rate in civilian sector jobs than their never-served counterparts when their job characteristics and work experience were similar. The critical issue is how to treat the period of time spent in the AVF by former enlistees. If these weeks are included as part of their civilian weeks of work (Tables 8.1 to 8.4, columns 1-4), then there is only fragmentary evidence that wages were responsive to work experience (black males), and no evidence that the pay discrepancy that favors the never-served would be completely eliminated in the future. The alternative assumption, that these weeks were not valued by civilian employers and should be included among the weeks of nonwork (Tables 8.1 to 8.4, column 5), leads to the findings that wages reflected civilian work (nonwork) experience, and that the wage advantage that never-served male youth enjoy would be reduced. Acceptance of the latter assumption also suggests a wage premium for male veterans in the future as their work histories and job characteristics approach those of the never-served males. The data are less supportive of the same conclusion for female veterans.

Hourly pay was not responsive to the number of different employers or unique occupational changes within the subgroups of out-of-school working youth. Wages were also unaffected by the magnitude of these turnovers under the two assignments of the weeks in the AVF. These results were not unexpected for at least two reasons. First, the bivariate tabulations generally showed a weak association between these variables. Second, the consequences of mobility are captured by other factors specified in the wage equations. Involuntary separations, particularly those with no recall, increase the weeks of nonwork as workers search for new jobs. Once reemployed, the likelihood of a subsequent layoff is also very high because of the low seniority of the worker. Additional periods of nonwork and employer changes will be observed until these workers find more stable work environments. Employer changes that were more voluntary also increased the employer counts but probably had a different effect on the work experience measures. Less frequent employer turnover, or a different time distribution of these changes, meant a longer period of attachment to an employer (tenure); and opportunities to change employers and work assignments, as well as the actual changes, are affected by the workers' race, sex, levels of education and achievement, health status and places of work.

The findings in this section are based on a sample of youth who had some work experience between their 1982 and 1983 interview dates. The further restriction of the sample to workers only at the time of the 1983 interviews did not alter any of the conclusions. Thus the expansion of the sample increased the size of the different subgroups without significantly affecting any of the results (Tables 8A.14-8A.18).

V. CONCLUSIONS

Decisions by youth about sector of employment, location, and other attributes of their jobs, and attachment to an employer affected their wages five to seven years after leaving school. Personal characteristics also impacted on how they were compensated hourly. Workers with comparable work experiences and backgrounds were not rewarded equally, however. White males who worked in firms where wages were set by collective bargaining agreements averaged 35 percent more in pay than their uncovered counterparts, while the corresponding premium for covered black males and all females was about 19 percent and 16 percent, respectively--the latter two gains about one-half the size of the former. Wages and tenure were unrelated for black male workers, but white males averaged about three percent more in pay per hour, and females earned a one and one-half percent premium for each 10-week period of work with the same employer. Additional education and achievement levels had no independent effect on male wages, but female compensation increased by about six percent with each year of regular school completed, and about eight percent for each 10-point increase in achievement score. Male wages did not differ by the race of worker, but hourly pay of Hispanic and black females exceeded the average wage of white female workers by about ten to 12 percent.

The introduction of the AVF in 1973 and the increased attractiveness of the armed services as an employer was a mixed blessing for out-of-school youth. The decision to enlist shortened the transition time between school and first job and reduced the loss in earnings because of periods of nonwork. Qualified youth who joined the AVF averaged about three months between the date they left school and the time they became active. Their never-served counterparts, however, averaged about 39 weeks to find a regular job in the civilian sector, and the female subgroup who never joined averaged more than one year before settling into regular jobs. The latter averages were also affected by the AVF's hiring practices; the new accessions each year eliminated these youth from the private sector pool of job applicants and thereby improved the likelihood of an earlier employee-employer match for those who remained.

Despite the AVF's attractiveness as an entry-level employer, some enlistees were at a significant pay disadvantage in their civilian jobs after discharge from the service. Female attriters and white males who previously served averaged about $1.50 less per hour in pay than their counterparts who never served. Former enlistees would be expected to earn less initially if civilian employers discounted their military experiences or valued it differently from civilian work. The higher average wage for non-enlistees can also be explained by the wage premiums from longer tenure and more extensive collective bargaining coverage (Table 8A.21). White male veterans and female attriters had significantly lower-than-average durations in their jobs and the white male veterans were the least likely of the male workers to have their wages determined by collective bargaining agreements. The wage discrepancy among white males is not expected to persist in the future as the veterans accumulate additional work experience and change employers and conditions of employment, and as employers acquire better information about the capabilities of these workers. If, in fact, these two groups of young male workers had identical profiles in 1983 the veteran would have averaged about 22 percent more in pay per hour. The very low wages of working female attriters is a more puzzling finding. These women had the highest achievement levels of any group of youth, and they were more likely than their never-served female counterparts to be in jobs with collective bargaining agreements. A more detailed analysis of this group is needed to ascertain some of the reasons for the large discrepancy in pay.

A number of other questions need additional research. Although we found little evidence that increases in occupational and employer counts affect wages, the timing of these changes, and not simply their magnitude, might be more important. A more detailed analysis of the occupational counts is also needed to establish their trajectories and institutional settings. Intrafirm promotions, for example, may be compensated more extensively than changes in occupations that also involve new employers. A further analysis of the military and post-military job assignments could explain why employers undervalue some of the time spent in the military. The decision to join the AVF, to change employers, or to switch occupations are choice variables that need to be modelled separately. The weeks of work measure excludes the intensity of the work effort. A more complete analysis of the association between wages and work experience requires that this dimension of the labor supply decision be studied.

Chapter 8

Notes

[1] Unemployment rates for black youth ages 18-19 in the period 1976-81 averaged between 32 and 39 percent; the corresponding averages for all youth of the same ages in this period varied between 14 and 18 percent (U.S. Department of Labor, 1982). Weeks of unemployment and weeks of nonwork are used interchangably for ease of exposition, even though they are different measures of nonwork status.

[2] The dichotomy is not so sharply defined because large numbers of youth work while attending classes.

[3] Black males born in 1959 or 1960 were about two times more likely to enter the AVF than white males from the same birth cohort years. Slightly better than one in five black males had enlisted by 1983 while the white likelihood was about one in eight. Corresponding female participation rates in the AVF were considerably smaller, with less than one in 50 whites and one in 25 blacks having joined by 1983. Overall, about one in 14 youths in these two birth cohort years served in the AVF by 1983.

[4] See note 3 above.

[5] The hourly earnings in the most recent job was selected if the respondent was not at work at the 1983 interview date but had some work experience since the last interview date.

[6] A regular job requires at least nine weeks of continuous work with the same employer and 20 or more hours of work weekly.

[7] See also Nestel, 1983.

[8] While an employer identification code is available for all jobs, information about occupation and industry is restricted to regular jobs only. These experiences are censored at both ends of the work histories. Detailed information about regular jobs is available for the period that begins in 1978 and ends with the interview week in 1983. Thus the work experiences of youth who left school after 1977 is complete, at least until mid-1983. Less complete information is available for youth who left school before 1978. The pre-1978 gaps are reduced considerably if the first regular job is a long-tenured one, or is eliminated if the first job extended beyond the first week in 1978.

[9] More accurately, it should vary with the number of potential weeks of work in the civilian sector.

[10] Civilian regular jobs held while in the military are included in these counts.

[11] The Glossary contains a more complete description of these variables.

[12] Appendix Tables 8A.14 - 8A.18 summarize these findings.

[13] The absence of a significant association could be explained by the restriction of the sample to youth with 10 to 14 years of school completed which limits the variation in the education and achievement measures.

[14] Calculated by $(e^{\alpha_i}-1)$ where α_1 is the coefficient of the dichotomous variable (Halvorsen, 1980).

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U.S. Department of Labor. 1982. 1982 Employment and Training Report of the President. Superintendent of Documents, U.S. Government Printing Office: Tables A-30-A.31, pp. 194-197.

Glossary

AFQT	The sum of the correct responses to the arithmetic reasoning, word knowledge, paragraph comprehension and numerical operations sections of the Armed Forces Aptitude Battery (ASVAB) test. One-half the score in the latter exam is added to the scores of the first three exams to arrive at the final AFQT score. AFQT scores range in value between 0 and 105.
ATTRITER	Dichotomous variable that equals one if enlistee left AVF before completing initial tour; zero otherwise.
BLACK	Dichotomous variable that equals one if respondent is black, zero otherwise.
EDUCATION	The number of years of regular school completed.
EMPLOYERS	Number of different civilian jobs held between date left school and date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.
EMPLOY SQ	The EMPLOYERS variable squared.
HEALTH	Dichotomous variable that equals one if health problems limit the amount or kind of work the respondent could perform; zero otherwise.
HISPANIC	Dichotomous variable that equals one if the respondent is Hispanic; zero otherwise.
LAMBDA	The ratio of the standard normal to cumulative normal distribution functions estimated from the probit equation of employment probability.
MARRIED	Dichotomous variable that equals one if respondent was married in survey week 1983; zero otherwise.
NEVER SERVED	Dichotomous variable that equals one if youth never served in AVF; zero otherwise.
OCCUPATIONS	Number of different kinds of work in regular jobs held between date left school and date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.
OCC SQ	The OCCUPATIONS variable squared.
SMSA	Dichotomous variable that equals one if the respondent resided in an SMSA in survey week 1982; zero otherwise.

SOUTH	Dichotomous variable that equals one if respondent resided in the South in survey week 1983; zero otherwise.
TENURE	The number of weeks respondent has worked for his 1983 survey week (last) employer.
UNION	Dichotomous variable that equals one if respondent's wages were set under a collective bargaining agreement; zero otherwise.
WAGE	Respondent's hourly rate of pay (in cents) in 1983 survey week (last) job.
WKS NONWORK	The number of nonworking weeks between date left school and 1983 survey week. Weeks in the AVF are excluded.
WKS NONWORK (REV)	The number of nonworking weeks between date left school and 1983 survey week. Weeks in the AVF are included.
WKS WORK	The number of weeks of paid work between date left school and 1983 survey week. Weeks in the AVF are included.
WKS WORK (REV)	The number of weeks of paid work between date left school and 1983 survey week. Weeks in the AVF are excluded.

Table 8A.1 Mean Weeks Between Date Left School and Date First Regular Post-School Job Started,[a] by Year Left School, Sex and Race, 1983[b]

	Male									
	White		Black		Total[e]		Female		Total	
Characteristic	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean
					1976					
Never served	135	17.9	46	34.0	210	20.2	322	54.3	532	40.1
Ever served	152	9.6	52	13.2	219	10.2	89	14.7	308	10.5
Completer[c]	124	10.6	44	15.7	178	11.7	67	10.4	245	11.6
Serving[d]	33	11.3	22	f	60	13.2	23	f	83	12.8
Not serving	91	10.3	22	f	118	11.0	44	11.8	162	11.1
Noncompleter	24	5.6	7	f	35	5.7	20	f	55	7.2
Completion status NA	4	f	1	f	6	f	2	f	8	f
Total	287	16.4	98	27.0	429	18.1	411	53.7	840	36.9
					1977					
Never served	188	11.3	71	46.8	305	16.3	372	47.9	677	32.9
Ever served	131	7.4	40	11.0	188	8.2	134	9.4	322	8.3
Completer[c]	107	7.5	29	8.1	152	7.7	94	12.8	246	8.1
Serving[d]	36	6.3	18	f	62	7.1	50	14.4	112	7.7
Not serving	71	8.3	11	f	90	8.2	44	11.3	134	8.4
Noncompleter	21	f	9	f	30	10.1	36	15.2	66	9.2
Completion status NA	3	f	2	f	6	f	4	f	10	f
Total	319	10.8	111	38.7	493	15.1	506	47.3	999	30.9
					1978					
Never served	248	34.4	104	44.1	416	36.5	536	58.4	952	48.1
Ever served	79	14.4	37	22.4	126	16.5	67	23.5	193	17.4
Completer[c]	48	11.5	15	f	69	15.1	44	16.4	113	15.2
Serving[d]	22	f	10	f	34	9.7	24	f	58	11.0
Not serving	26	13.6	5	f	35	19.2	20	f	55	18.7
Noncompleter	21	f	11	f	34	13.5	21	f	55	17.1
Completion status NA	10	f	11	f	23	20.3	2	f	25	19.9
Total	327	32.1	141	39.4	542	33.9	603	57.7	1145	45.8
					Total					
Never served	571	22.3	221	42.5	931	25.6	1230	53.9	2161	41.0
Ever served	362	10.5	129	16.1	533	11.6	290	17.0	823	12.2
Completer[c]	279	9.6	88	15.9	399	10.9	205	12.8	604	11.0
Serving[d]	91	8.4	50	13.7	156	9.8	97	14.6	253	10.2
Not serving	188	10.2	38	18.2	243	11.6	108	11.0	351	11.5
Noncompletion	66	8.3	27	13.7	99	9.5	77	20.6	176	11.3
Completion status NA	17	f	14	f	35	18.8	8	f	43	18.5
Total	933	20.7	350	35.8	1464	23.4	1520	53.3	2984	38.5

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[a] Youth with first post-school regular jobs that are a continuation of a school job are assumed to have had no interruption in work experience after leaving school. The earlier beginning date of a first job that began after 1977 is set to January 1, 1978.

[b] Youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[c] Includes former enlistees who were within two months of completing their first tour at time of discharge.

[d] Includes enlistees in a second or later tour.

[e] Includes Hispanic male youth.

[f] Less than 25 sample cases.

Table 8A.2 Year First Post-School Regular Job Started, by Civilian and Military Status, Year Left School, and Sex: 1983[a]

Characteristic	Sample cases[b]	Total percent	Year first job began							
			Prior to leaving school	1976	1977	1978	1979	1980	1981	1982-1983
Male										
1976										
Never served	216	100.0	28.8	44.8	18.2	6.5	1.1	0.5	0.1	0.0
Ever served	222	100.0	16.1	71.0	11.4	0.7	0.0	0.9	0.0	0.0
Prior civilian job[c]	75	100.0	43.0	43.2	12.5	1.3	0.0	0.0	0.0	0.0
No prior civilian job[d]	147	100.0	0.0	87.5	10.7	0.4	0.0	1.4	0.0	0.0
Total	438	100.0	26.2	50.2	16.8	5.3	0.9	0.6	0.1	0.0
1977										
Never served	308	100.0	41.9	--	41.8	11.6	3.3	0.7	0.2	0.4
Ever served	191	100.0	8.6	--	84.8	6.7	0.0	0.0	0.0	0.0
Prior civilian job[c]	47	100.0	30.1	--	59.9	10.0	0.0	0.0	0.0	0.0
No prior civilian job[d]	144	100.0	0.0	--	94.7	5.3	0.0	0.0	0.0	0.0
Total	499	100.0	37.1	--	48.0	10.9	2.8	0.6	0.1	0.4
1978										
Never served	416	100.0	38.4	--	--	42.7	14.8	2.1	0.4	1.5
Ever served	129	100.0	11.6	--	--	67.3	18.8	0.0	2.3	0.0
Prior civilian job[c]	24	100.0	e	--	--	e	e	e	e	e
No prior civilian job[d]	105	100.0	0.0	--	--	74.2	22.2	0.0	3.6	0.0
Total	545	100.0	34.6	--	--	46.1	15.4	1.8	0.7	1.3
Female										
1976										
Never served	323	100.0	19.2	44.4	17.6	7.8	7.4	1.8	0.4	1.3
Ever served	91	100.0	10.4	80.7	7.2	1.7	0.0	0.0	0.0	0.0
Prior civilian job[c]	49	100.0	27.8	70.9	1.2	0.0	0.0	0.0	0.0	0.0
No prior civilian job[d]	42	100.0	0.0	86.6	10.7	2.7	0.0	0.0	0.0	0.0
Total	414	100.0	19.1	45.0	17.5	7.7	7.3	1.8	0.4	1.3
1977										
Never served	374	100.0	25.0	--	46.6	17.1	6.7	1.4	1.9	1.3
Ever served	136	100.0	41.1	--	46.4	12.5	0.0	0.0	0.0	0.0
Prior civilian job[c]	46	100.0	71.5	--	26.1	2.3	0.0	0.0	0.0	0.0
No prior civilian job[d]	90	100.0	0.0	--	73.8	26.2	0.0	0.0	0.0	0.0
Total	510	100.0	25.3	--	46.6	17.0	6.6	1.4	1.8	1.3
1978										
Never served	536	100.0	29.2	--	--	42.7	20.1	4.6	1.9	1.5
Ever served	73	100.0	0.0	--	--	66.2	33.8	0.0	0.0	0.0
Prior civilian job[c]	4	100.0	e	--	--	e	e	e	e	e
No prior civilian job[d]	69	100.0	0.0	--	--	48.0	52.0	0.0	0.0	0.0
Total	609	100.0	28.5	--	--	43.3	20.5	4.5	1.8	1.4
Total										
1 76										
Never served	539	100.0	23.4	44.6	17.9	7.3	4.7	1.2	0.3	0.7
Ever served	313	100.0	15.7	71.7	11.1	0.8	0.0	0.8	0.0	0.0
Prior civilian job[c]	124	100.0	41.9	45.2	11.6	1.2	0.0	0.0	0.0	0.0
No prior civilian job[d]	189	100.0	0.0	87.5	10.7	0.6	0.0	1.3	0.0	0.0
Total	852	100.0	22.5	47.5	17.1	6.6	4.2	1.2	0.2	0.7
1977										
Never served	682	100.0	33.3	--	44.2	14.4	5.0	1.1	1.0	0.9
Ever served	327	100.0	11.8	--	80.9	7.2	0.0	0.0	0.0	0.0
Prior civilian job[c]	93	100.0	37.7	--	53.7	8.6	0.0	0.0	0.0	0.0
No prior civilian job[d]	234	100.0	0.0	--	93.4	6.6	0.0	0.0	0.0	0.0
Total	1009	100.0	31.5	--	47.3	13.8	4.6	1.0	1.0	0.8

Table 8A.2 (continued)

	Year first job began									
Characteristic	Sample cases[b]	Total percent	Prior to leaving school	1976	1977	1978	1979	1980	1981	1982-1983
1978										
Never served	952	100.0	33.7	--	--	42.7	17.5	3.4	1.2	1.5
Ever served	202	100.0	10.0	--	--	67.2	20.9	0.0	2.0	0.0
Prior civilian job[c]	28	100.0	25.8	--	--	60.7	13.5	0.0	0.0	0.0
No prior civilian job[d]	174	100.0	0.0	--	--	71.2	25.6	0.0	3.2	0.0
Total	1154	100.0	31.7	--	--	44.8	17.8	3.1	1.2	1.4

[a] Youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Includes youth with post-school work with a civilian employer of at least nine weeks duration and 20 hours or more of work weekly (i.e., regular job).

[c] Includes youth who did not join the military in the year they left school and who worked in a civilian job 20 hours or more weekly for at least nine weeks before entry.

[d] Includes youth who entered the military in the same year they left school or entered in a later year but had no regular civilian job prior to entry.

[e] Less than 25 sample cases.

Table 8A.3 Mean Number of Post-School Weeks Worked, by Civilian and Military Status, Sex and Race: 1983[a]

	Male									
	White		Black		Total[b]		Female		Total	
Characteristic	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean
Weeks worked[d]										
Never served	547	237	210	192	890	231	1149	189	2039	209
Previously served	275	239	75	224	377	236	209	154	586	227
Veteran[c]	206	259	45	242	271	255	123	202	394	252
Attriter	69	214	30	202	106	213	86	130	192	200
Total	825	236	285	198	1270	231	1360	189	2630	209
Weeks worked[e]										
Never served	547	237	210	192	890	231	1149	189	2039	209
Previously served	275	138	75	96	377	129	209	101	586	126
Veteran[c]	206	122	45	69	271	110	123	113	394	110
Attriter	69	158	30	128	106	153	86	95	192	144
Total	825	225	285	175	1270	218	1360	188	2630	203

[a] Civilian youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Includes Hispanic male youth.

[c] Includes former enlistees who were within two months of completing their first tour at time of discharge.

[d] Includes weeks of military service.

[e] Excludes weeks of military service.

Table 8A.4 Mean Number of Post-School Civilian Employers,[a] 1978-1983, by Civilian and Military Status, Sex, and Race: 1983[b]

	Male									
	White		Black		Total[d]		Female		Total	
Characteristic	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean
Total, total[e]										
Never served	579	7.8	218	6.9	933	7.7	1141	6.7	2074	7.2
Previously served	270	5.5	69	5.0	365	5.4	186	4.7	551	5.3
Veteran[c]	199	3.8	41	3.4	259	3.7	104	2.3	363	3.6
Attriter	68	7.2	28	6.6	103	7.1	80	5.7	183	6.9
Total	849	7.6	287	6.6	1298	7.5	1327	6.7	2625	7.1
Total, different[f]										
Never served	576	4.1	216	3.9	926	4.1	1132	3.6	2058	3.9
Previously served	269	3.8	68	3.2	363	3.7	184	3.1	547	3.6
Veteran[c]	199	2.6	41	2.1	259	2.6	103	1.7	362	2.5
Attriter	67	5.0	27	4.4	101	4.9	79	3.8	180	4.7
Total	845	4.1	284	3.8	1289	4.1	1316	3.6	2605	3.8
Regular, total[g]										
Never served	579	6.4	215	5.7	930	6.3	1130	5.5	2060	5.9
Previously served	267	4.2	69	4.1	362	4.2	179	3.3	541	4.1
Veteran[c]	197	3.0	41	2.8	257	2.9	98	2.0	355	2.9
Attriter	67	5.4	28	5.5	102	5.4	79	3.9	181	5.2
Total	846	6.1	284	5.4	1292	6.0	1309	5.5	2601	5.8
Regular, different[h]										
Never served	574	3.2	214	3.1	922	3.2	1118	3.0	2040	3.1
Previously served	265	2.9	68	2.7	359	2.8	178	2.1	537	2.8
Veteran[c]	197	2.0	41	1.7	257	2.0	97	1.5	354	2.0
Attriter	65	3.8	27	3.6	99	3.8	79	2.4	178	3.6
Total	839	3.2	282	3.0	1281	3.2	1296	2.9	2577	3.1

[a] Excludes all post-school jobs that ended prior to January 1, 1978.

[b] Civilian youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[c] Includes former enlistees who were within two months of completing their first tour at time of discharge.

[d] Includes Hispanic male youth.

[e] No restriction on the duration worked on the job, or the number of hours worked weekly. Youth who were with the same employer in more than one survey period are counted as having worked with different employers in the two periods.

[f] Count of different employers in jobs held in a reference survey period and those held in the survey period immediately preceding.

[g] Count of employers in jobs held that extended for at least nine weeks with at least 20 hours of work weekly.

[h] Count of different employers in regular jobs held in a reference survey period and those held in the survey period immediately preceding.

Table 8A.5 Mean Number of Post-School Civilian Occupations, 1978-1983,[a] by Civilian and Military Status, Sex and Race: 1983[b]

Characteristic	Male						Female		Total	
	White		Black		Total[d]					
	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean
Regular, total[e]										
Never served	579	6.3	215	5.6	930	6.2	1130	5.4	2060	5.8
Previously served	267	4.2	69	4.1	362	4.2	179	3.2	541	4.1
Veteran[c]	197	2.9	41	2.8	257	2.9	98	1.9	355	2.9
Attriter	67	5.4	28	5.5	102	5.4	79	3.7	181	5.1
Total	846	6.1	284	5.4	1292	6.0	1309	5.4	2601	5.7
Regular, different[f]										
Never served	579	4.0	215	4.0	930	4.0	1130	3.5	2060	3.8
Previously served	267	3.3	69	2.9	362	3.2	179	2.5	541	3.1
Veteran[c]	197	2.4	41	2.2	257	2.4	98	1.7	355	2.3
Attriter	67	4.0	28	3.7	102	3.9	79	2.8	181	3.7
Total	846	4.0	284	3.8	1292	3.9	1309	3.5	2601	3.7

[a] Excludes all post-school regular jobs that ended prior to January 1, 1978

[b] Civilian youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[c] Includes former enlistees who were within two months of completing their first tour at time of discharge.

[d] Includes Hispanic male youth.

[e] Restricted to jobs that extended for at least nine weeks with an employer and 20 hours or more of work weekly. Similar kinds of work done in different job settings (employers) are counted as different occupations. NLS occupation codes based on 1970 Census occupation classification.

[f] Restricted to unique occupations in civilian regular jobs held since leaving school.

Table 8A.6 Mean Number of Post-School Civilian Industries, 1978-1983,[a] by Civilian and Military Status, Sex and Race: 1983[b]

Characteristic	Male						Female		Total	
	White		Black		Total[d]					
	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean
Regular, total[e]										
Never served	579	6.3	215	5.6	930	6.2	1130	5.4	2060	5.8
Previously served	267	4.2	69	4.1	362	4.2	179	3.2	541	4.1
Veteran[c]	197	2.9	41	2.8	257	2.9	98	1.9	355	2.9
Attriter	67	5.4	28	5.5	102	5.4	79	3.7	181	5.1
Total	846	6.1	284	5.4	1292	6.0	1309	5.4	2601	5.7
Regular, different[f]										
Never served	579	3.6	215	3.4	930	3.6	1130	2.9	2060	3.3
Previously served	267	3.0	69	3.1	362	3.0	179	2.1	541	2.9
Veteran[c]	197	2.3	41	2.2	257	2.3	98	1.5	355	2.2
Attriter	67	3.7	28	3.9	102	3.8	79	2.3	181	3.5
Total	846	4.7	284	3.3	1292	3.5	1309	2.9	2601	3.2

[a] Excludes all post-school regular jobs that ended prior to January 1, 1978

[b] Civilian youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[c] Includes former enlistees who were within two months of completing their first tour at time of discharge.

[d] Includes Hispanic male youth.

[e] Restricted to jobs that extended for at least nine weeks with an employer and 20 hours or more of work weekly. Jobs that belong to the same industrial classification are counted as different industries. NLS industry codes based on 1970 Census industrial classification.

[f] Restricted to unique industries in civilian regular jobs held since leaving school.

Table 8A.7 Mean Wage in 1983 Survey Week (Last) Job, by Number of Post-School Civilian Employers, 1978-1983, Sex, and Race: 1983[a]

(in dollars)

Characteristic	Male						Female		Total	
	White		Black		Total[f]					
	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean
Total, total[b]										
1	18	e	5	e	27	$ 6.14	38	$ 4.08	65	$ 5.22
2	33	$ 6.26	9	e	51	5.65	55	3.95	106	4.66
3	52	5.80	16	e	72	5.29	64	3.85	136	4.39
4	52	6.27	24	e	80	6.04	77	4.65	157	5.22
5	111	8.08	41	$ 5.83	166	7.74	173	5.51	339	6.65
6	106	7.95	37	7.02	171	7.81	147	5.09	318	6.54
7	89	6.68	26	5.14	128	6.58	137	5.59	265	6.08
8	96	7.30	22	e	136	7.17	99	4.70	235	6.13
9	59	6.65	18	e	94	6.50	85	5.51	179	6.06
10	45	6.95	12	e	69	6.77	46	5.24	115	6.09
11-15	78	6.58	24	e	119	6.47	99	5.29	218	5.95
16 +	19	e	2	e	25	5.85	16	e	41	5.32
Total	758	7.14	236	5.65	1138	6.95	1036	5.13	2174	6.09
Regular, total[c]										
1	32	$ 6.03	6	e	44	$ 5.74	48	$ 3.81	92	$ 4.89
2	56	5.95	18	e	82	5.38	77	3.87	159	4.55
3	64	6.09	19	e	88	5.62	80	4.17	168	4.81
4	66	5.79	24	e	103	5.75	113	4.96	216	5.30
5	151	7.83	56	$ 6.69	225	7.67	215	5.33	440	6.58
6	139	7.53	47	5.48	222	7.33	195	5.13	417	6.28
7	111	7.49	27	6.03	163	7.35	137	5.34	300	6.44
8	61	6.28	18	e	98	6.16	84	5.89	182	6.04
9	43	6.35	13	e	64	6.28	46	4.62	110	5.61
10	21	e	4	e	26	7.17	23	e	49	6.01
11-15	14	e	4	e	23	e	18	e	41	6.02
Total	758	7.14	236	5.65	1138	6.95	1036	5.13	2174	6.09
Total, different[d]										
1	110	$ 8.16	36	$ 5.67	165	$ 7.82	190	$ 5.51	355	$ 6.64
2	161	7.78	51	6.57	236	7.61	216	4.98	452	6.30
3	122	6.76	48	4.78	189	6.51	186	5.38	375	5.95
4	110	7.13	31	6.08	166	7.02	124	4.59	290	6.06
5	86	6.78	23	e	129	6.66	109	5.01	238	5.88
6	57	6.93	11	e	78	6.71	79	5.16	157	5.94
7	45	6.07	15	e	68	5.82	46	5.06	114	5.48
8	23	e	6	e	36	7.43	26	6.09	62	6.93
9	14	e	5	e	21	e	20	e	41	5.79
10	4	e	5	e	11	e	14	e	25	4.72
11-15	18	e	3	e	26	5.27	18	e	44	4.84
16 +	5	e	0	e	7	e	3	e	10	e
Total	755	7.13	234	5.65	1132	6.95	1031	5.13	2163	6.09
Regular, different										
1	154	$ 8.07	52	$ 6.26	228	$ 7.79	242	$ 5.47	470	$ 6.63
2	199	7.47	53	5.83	284	7.30	241	4.91	525	6.21
3	136	6.83	48	5.15	210	6.64	203	5.28	413	5.94
4	111	6.51	40	5.64	178	6.41	147	4.81	325	5.68
5	71	6.62	19	e	113	6.43	103	5.17	216	5.83
6	44	7.40	1	e	56	7.32	43	4.73	99	6.31
7	22	e	10	e	34	6.06	31	5.30	65	5.69
8	9	e	3	e	14	e	10	e	24	e
9	3	e	2	e	7	e	7	e	14	e
10	3	e	0	e	4	e	1	e	5	e
11+	1	e	0	e	2	e	1	e	3	e
Total	753	7.12	234	5.65	1130	6.94	1029	5.13	2159	6.08

Table 8A.7 (continued)

[a] Civilian youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] No restriction on the duration of the job or the number of hours worked weekly. Youth who remain with the same employer beyond a survey year, or who return to an earlier employer in a subsequent survey year, are counted as having worked with different employers in each of the years. Excludes all work experience ending before January 1, 1978.

[c] Periods of work with an employer that lasted at least nine weeks with 20 hours or more of work weekly. Excludes work experience ending before January 1, 1978.

[d] Restricted to comparison of employer in jobs held during a survey period and in job held in the survey period immediately proceeding. Excludes work experience ending before January 1, 1978.

[e] Less than 25 sample cases.

[f] Includes Hispanic male youth.

Table 8A.8 Mean Wage in 1983 Survey Week (Last) Job, by Number of Post-School Civilian Occupations, 1978-1983, by Race and Sex: 1983[a]
(in dollars)

	Male									
	White		Black		Total[e]		Female		Total	
Characteristic	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean
Regular, total[b]										
1	32	$ 6.03	6	d	44	$ 5.74	48	$ 3.81	92	$ 4.89
2	57	5.92	18	d	83	5.37	83	3.75	166	4.41
3	65	6.10	19	d	89	5.63	76	4.28	165	4.91
4	70	5.95	25	$ 5.48	108	5.86	114	5.02	222	5.40
5	149	7.94	59	6.51	227	7.73	217	5.34	444	6.62
6	142	7.36	44	5.65	223	7.22	202	5.22	425	6.25
7	109	7.50	28	6.05	163	7.35	132	5.20	295	6.40
8	61	6.37	16	d	94	6.24	89	5.70	183	5.98
9	40	6.38	13	d	60	6.31	37	4.84	97	5.80
10	20	d	5	d	27	7.07	23	d	50	5.84
11 +	13	d	3	d	20	d	15	d	35	6.13
Total	758	7.14	236	5.65	1138	6.95	1036	5.13	2174	6.09
Regular, different[c]										
1	77	$ 7.50	18	d	105	$ 7.08	133	$ 4.67	238	$ 5.75
2	126	7.48	40	$ 6.94	188	7.33	208	5.14	396	6.12
3	184	7.40	49	5.24	265	7.21	197	5.30	462	6.38
4	142	6.73	45	5.47	222	6.59	214	5.23	436	5.91
5	102	7.25	43	5.84	166	7.05	150	4.99	316	6.11
6	74	6.84	24	d	114	6.68	81	5.40	195	6.18
7	33	6.87	8	d	46	6.81	36	4.85	82	5.99
8	18	d	5	d	25	6.90	10	d	35	6.53
9	2	d	3	d	6	d	6	d	12	d
10	0	d	0	d	0	d	1	d	1	d
11 +	0	d	1	d	1	d	0	d	1	d
Total	758	7.14	236	5.65	1138	6.95	1036	5.13	2174	6.09

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[a] Civilian youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Periods of work with an employer that extended for at least nine weeks with 20 hours or more of work weekly. Excludes work experience ending before January 1, 1978.

[c] Based on a comparison of 3-digit occupation codes in all post-school civilian regular jobs held since January 1, 1978.

[d] Less than 25 sample cases.

[e] Includes Hispanic male youth.

Table 8A.9 Mean Wage in 1983 Survey Week (Last) Job, by Number of Post-School Civilian Industries, 1978-1983, Sex and Race: 1983[a]

(in dollars)

	Male									
	White		Black		Total[e]		Female		Total	
Characteristic	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean
Regular, total[b]										
1	32	$ 6.03	6	e	44	$ 5.74	48	$ 3.81	92	$ 4.89
2	57	5.92	18	e	83	5.37	81	3.88	164	4.51
3	65	6.10	19	e	90	5.74	78	4.15	168	4.87
4	70	6.33	25	$ 5.48	107	6.10	116	5.11	223	5.55
5	153	7.84	60	6.48	233	7.64	217	5.33	450	6.57
6	140	7.32	43	5.65	218	7.20	206	5.22	424	6.22
7	108	7.60	28	6.07	163	7.43	134	5.14	297	6.41
8	63	6.30	17	d	98	6.19	82	5.81	180	6.02
9	38	6.46	13	d	57	6.34	37	4.73	94	5.78
10	20	d	5	d	27	7.09	24	d	51	5.89
11 +	12	d	2	d	18	d	13	d	31	5.92
Total	758	7.14	236	5.65	1138	6.95	1036	5.13	2174	6.09
Regular, different[c]										
1	102	$ 7.52	36	$ 6.72	154	$ 7.38	206	$ 5.33	360	$ 6.16
2	156	7.43	43	4.73	235	7.09	266	4.96	501	5.90
3	179	7.48	51	6.01	254	7.34	214	5.04	468	6.36
4	139	7.07	47	5.40	213	6.87	168	5.18	381	6.08
5	82	6.19	30	5.77	138	6.16	106	5.49	244	5.86
6	58	6.79	16	d	82	6.64	47	4.77	129	5.98
7	29	7.48	9	d	43	7.15	21	d	64	6.53
8	10	d	2	d	14	d	7	d	21	d
9	2	d	2	d	4	d	1	d	5	d
10 +	1	d	0	d	1	d	0	d	1	d
Total	758	7.14	236	5.65	1138	6.95	1036	5.13	2174	6.09

[a] Civilian youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Periods of work with an employer that lasted at least nine weeks with at least 20 hours of work weekly. Excludes all work experience ending before January 1, 1978.

[c] Based on a comparison of 3-digit industry codes in all post-school civilian regular jobs held since January 1, 1978.

[d] Less than 25 sample cases.

[e] Includes Hispanic male youth.

Table 8A.10 Labor Force and Employment Status in 1983 Survey Week, by Civilian and Military Status, Sex, and Race: 1983[a]

(percentage distributions)

Characteristic	Sample size	Total percent	In labor force			Not in labor force
			Employed	Unemployed	Total	
White male						
Never served	579	100.0	78.4	15.2	93.6	6.4
Previously served[b]	276	100.0	62.2	23.6	85.8	14.2
Veteran[c]	207	100.0	66.8	21.5	88.3	11.7
Attriter	69	100.0	56.7	26.2	82.9	17.1
Total	858	100.0	76.8	16.0	92.8	7.2
Black male						
Never served	222	100.0	64.4	25.4	89.8	10.2
Previously served[b]	75	100.0	64.3	24.4	88.7	11.3
Veteran[c]	45	100.0	65.3	28.6	94.0	6.0
Attriter	30	100.0	63.0	19.3	82.3	17.7
Total	297	100.0	64.4	25.2	89.6	10.4
Total male[d]						
Never served	940	100.0	76.5	16.4	93.0	7.0
Previously served[b]	378	100.0	63.1	23.5	86.5	13.5
Veteran[c]	272	100.0	66.7	22.3	88.9	11.1
Attriter	106	100.0	58.6	25.0	83.6	16.4
Total	1321	100.0	75.1	17.2	92.3	7.7
Female						
Never served	1233	100.0	63.8	9.2	73.0	27.0
Previously served[b]	209	100.0	50.1	7.7	57.8	42.2
Veteran[c]	123	100.0	43.5	19.3	62.8	37.2
Attriter	86	100.0	53.4	1.8	55.2	44.8
Total	1444	100.0	63.6	9.2	72.8	27.2
Total						
Never served	2173	100.0	69.7	12.5	82.2	17.8
Previously served[b]	587	100.0	61.7	21.8	83.4	16.6
Veteran[c]	395	100.0	65.1	22.1	87.1	12.9
Attriter	192	100.0	57.8	21.4	79.2	20.8
Total	2765	100.0	69.2	13.1	82.3	17.7

[a] Civilian youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Includes former enlistees whose completion status in first job is not known.

[c] Includes former enlistees who were within two months of completing their first tour at time of discharge.

[d] Includes Hispanic male youth.

Table 8A.11 Mean Wage[a] in 1983 Survey Week (Last) Job, by Civilian and Military Status, Sex and Race: 1983[b]

(in dollars)

	Male									
	White		Black		Total[d]		Female		Total	
Characteristic	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean
Never served	515	$ 7.29	178	$ 5.65	814	$ 7.10	892	$ 5.14	1706	$ 6.12
Previously served	240	5.87	58	5.66	321	5.83	142	4.03	463	5.67
Veteran[c]	182	5.88	35	5.49	235	5.82	77	5.44	312	5.80
Attriter	58	5.87	23	e	86	5.84	65	3.52	151	5.52
Total	758	7.14	236	5.65	1138	6.95	1036	5.13	2174	6.09

[a] Wage and salary workers only.

[b] Civilian youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 uears of schooling completed and who had some work experience between 1982 and 1983 interview dates.

[c] Includes former enlistees who were within two months of completing their first tour at time of discharge.

[d] Includes Hispanic male youth.

[e] Less than 25 sample cases.

Table 8A.12 Mean AFQT Score, by Civilian and Military Status, Year Left School, Sex and Race: 1983[a]

	Male									
	White		Black		Total[d]		Female		Total	
Characteristic	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean
					1976					
Never served	129	71.4	44	47.7	197	68.3	313	68.8	510	68.6
Ever served	146	73.4	47	56.0	206	69.7	87	86.8	293	71.0
Completer[b]	118	78.0	41	56.3	167	72.2	67	81.4	234	72.7
Serving[c]	31	77.4	19	e	55	67.2	20	e	75	68.1
Not serving	87	78.2	22	e	112	74.4	47	81.6	159	74.9
Noncompleter	24	e	6	e	34	63.6	19	e	53	66.7
Completion status NA	4	e	0	e	5	e	1	e	6	e
Total	275	71.8	91	50.4	403	68.6	400	69.0	803	68.8
					1977					
Never served	174	76.5	70	47.4	284	72.4	367	68.9	651	70.6
Ever served	127	76.3	43	61.6	189	72.4	136	81.7	325	73.5
Completer[b]	106	79.4	32	62.3	156	74.7	94	77.2	250	74.9
Serving[c]	38	80.8	19	e	66	75.1	50	78.2	116	75.3
Not serving	68	78.7	13	e	90	74.3	44	76.3	134	74.5
Noncompleter	19	e	8	e	28	68.3	38	86.4	66	72.2
Completion status NA	2	e	3	e	5	e	4	e	9	e
Total	301	76.5	113	50.8	473	72.4	503	69.2	976	70.8
					1978					
Never served	238	72.4	100	42.4	396	68.0	518	69.6	914	68.9
Ever served	93	75.7	36	60.7	138	71.9	78	76.4	216	72.6
Completer[b]	62	77.1	14	e	81	73.9	54	65.9	135	72.9
Serving[c]	26	76.7	7	e	34	74.5	25	65.6	59	73.2
Not serving	36	77.4	7	e	47	73.5	29	66.2	76	72.7
Noncompleter	22	e	13	e	37	70.9	22	e	59	72.0
Completion status NA	9	e	9	e	20	e	2	e	22	e
Total	331	72.8	136	46.7	534	68.5	596	69.8	1130	69.2
					Total					
Never served	541	73.5	214	45.2	877	69.5	1198	69.1	2075	69.3
Ever served	366	75.0	126	59.5	533	71.3	301	80.4	834	72.3
Completer[b]	286	78.3	87	60.4	404	73.5	215	74.6	619	73.6
Serving[c]	95	78.6	45	59.2	155	72.2	95	74.5	250	72.4
Not serving	191	78.2	42	61.4	249	74.2	120	74.7	369	74.2
Noncompletion	65	69.8	27	56.8	99	67.6	79	83.3	178	70.2
Completion status NA	15	e	12	e	30	70.0	7	e	37	72.1
Total	907	73.7	340	48.9	1410	69.8	1499	69.4	2909	69.6

[a] Youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Includes former enlistees who were within two months of completing their first term at time of discharge.

[c] Includes enlistees in a second or later tour.

[d] Includes male Hispanic youth.

[e] Less than 25 sample cases.

Table 8A.13 Mean Wage[a] in First Post-School Job, by Year First Job Began, 1976-78, Civilian and Military Status, Sex and Race: 1983[b]

	Male									
	White		Black		Total[e]		Female		Total	
Characteristic	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean
1976										
Never served	60	$ 3.24	17	f	87	$ 3.18	111	$ 2.57	198	$ 2.85
Ever served[c]	126	3.22	37	$ 3.53	175	3.28	75	2.95	250	3.25
Prior civilian job[d]	36	3.26	11	f	53	3.47	44	2.54	97	3.37
No prior civilian job	90	3.20	26	3.20	122	3.20	31	3.20	153	3.20
Total	186	3.23	54	3.03	262	3.21	186	2.58	448	2.92
1977										
Never served	81	$ 2.98	20	f	119	$ 3.00	112	$ 2.89	231	$ 2.95
Ever served[c]	123	3.33	43	$ 3.54	181	3.35	92	3.20	273	3.35
Prior civilian job[d]	16	f	6	f	25	3.48	20	f	45	3.43
No prior civilian job	107	3.34	37	3.34	156	3.33	72	3.34	228	3.33
Total	204	3.06	63	3.40	300	3.10	204	2.90	504	3.02
1978										
Never served	177	$ 4.00	58	$ 3.45	282	$ 3.93	278	$ 3.08	560	$ 3.53
Ever served[c]	80	3.51	29	3.73	117	3.56	89	3.24	206	3.52
Prior civilian job[d]	12	f	6	f	20	f	3	f	23	f
No prior civilian job	68	3.55	23	f	97	3.55	86	3.55	183	3.55
Total	257	3.94	88	3.49	400	3.88	367	3.09	767	3.53
Total										
Never served	318	$ 3.57	95	$ 3.24	488	$ 3.54	501	$ 2.90	989	$ 3.23
Ever served[c]	329	3.35	109	3.60	473	3.39	256	3.14	729	3.37
Prior civilian job[d]	64	3.35	23	f	98	3.51	67	2.86	165	3.42
No prior civilian job	265	3.35	86	3.37	375	3.34	189	3.37	564	3.35
Total	647	3.53	205	3.36	962	3.51	757	2.91	1719	3.25

[a] Restricted to youth with first post-school regular jobs that begin in one of the years, 1976-78. Excludes youth whose first post-school regular civilian job began prior to January 1, 1978 and ended after that date because the earnings reported in 1979 were more likely to reflect the compensation at that time (if still in first job) or at the end of the job (if first job had terminated) than at the beginning date. The entry wage for youth whose first post-school regular job was the AVF is estimated by the monthly regular military compensation (RMC) of an E-1 enlistee in that year and the assumption of 40 hours of work weekly and 4 1/3 weeks in a month.

[b] Youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[c] Includes enlistees serving in the armed forces when interviewed in 1983.

[d] Post-school regular civilian job that preceeded entry into the AVF.

[e] Includes Hispanic male youth.

[f] Less than 25 sample cases.

Table 8A.14 (ln) Wage Regressions, Male Youth: 1983[a]

(t-ratios in parentheses)

Characteristic	Specification 1	2	3	4	5
AFQT	0.0010	0.0010	0.0010	0.0010	0.0012
	(0.84)	(0.79)	(0.78)	(0.82)	(0.93)
EDUCATION	-0.0301	-0.0321	-0.0325	-0.0312	-0.0298
	(-1.27)	(-1.35)	(-1.36)	(-1.30)	(-1.24)
SOUTH	-0.1094	-0.1084	-0.1090	-0.1066	-0.0956
	(-1.99)**	(-1.97)**	(-1.97)**	(-1.91)*	(-1.72)*
HISPANIC	0.0122	0.0141	0.0137	0.0147	0.0133
	(0.25)	(0.29)	(0.28)	(0.30)	(0.27)
BLACK	-.0226	-0.0269	-0.0261	-0.0282	-0.0308
	(-0.43)	(-0.51)	(-0.49)	(-0.53)	(-0.58)
SMSA	0.0498	0.0506	0.0502	0.0520	0.0629
	(1.20)	(1.22)	(1.21)	(1.24)	(1.50)
UNION	0.2879	0.2857	0.2856	0.2849	0.2827
	(8.47)**	(8.39)**	(8.38)**	(8.35)**	(8.30)**
NEVER SERVED	0.0224	0.0291	0.0306	0.0321	-0.2312
	(0.42)	(0.55)	(0.57)	(0.59)	(-3.26)**
ATTRITER	0.0138	0.0270	0.0272	0.0312	-0.1028
	(0.21)	(0.40)	(0.40)	(0.46)	(-1.40)
TENURE	0.0019	0.0017	0.0017	0.0017	0.0016
	(3.23)**	(2.82)**	(2.82)**	(2.80)**	(2.67)**
WKS WORK	0.0003	0.0003	0.0003	0.0002	
	(0.83)	(0.74)	(0.73)	(0.71)	
WKS NONWORK	-0.0012	-0.0012	-0.0012	-0.0012	
	(-2.80)**	(-2.80)**	(-2.80)**	(-2.76)**	
LAMBDA	-0.7150	-0.7197	-0.7220	-0.7110	-0.6776
	(-2.92)**	(-2.94)**	(-2.94)**	(-2.88)**	(-2.74)**
HEALTH	0.3620	0.3642	0.3660	0.3601	0.3450
	(2.20)**	(2.21)**	(2.22)**	(2.17)**	(2.08)**
EMPLOYERS[b]		-0.0063	-.0056	-0.0150	-0.0149
		(-1.08)	(-0.78)	(-0.86)	(-0.86)
OCCUPATIONS[c]			-0.0019	-0.0039	-0.0059
			(-0.17)	(-0.11)	(-0.17)
EMPLOY SQ				0.0007	0.0007
				(0.57)	(0.60)
OCC SQ				0.0004	0.0003
				(0.10)	(0.09)
WKS WORK (REV)[d]					0.0004
					(1.14)
WKS NONWORK (REV)[e]					-0.0011
					(-2.77)**
INTERCEPT	6.8177	6.8834	6.8943	6.8916	7.0768
	(14.98)**	(14.99)**	(14.86)**	(14.60)**	(15.05)**
$\bar{R}^2$	0.221	0.221	0.220	0.219	0.223
Sample cases	748	748	748	748	748

Table 8A.14 (continued)

[a] Male youth at work as civilian wage and salary earners in survey week 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Number of different civilian jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[c] Number of different kinds of civilian work in regular jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[d] Excludes weeks of military service for youth who previously served.

[e] Includes weeks of military service for youth who previously served.

* Significant at $\alpha = 0.05$, one-tail test.
**Significant at $\alpha = 0.025$, one-tail test.

Table 8A.15 (ln) Wage Regressions, White Male Youth: 1983[a]

(t-ratios in parentheses)

Characteristic	Specification				
	1	2	3	4	5
AFQT	-0.0003 (-0.16)	-0.0005 (-0.25)	-0.0005 (-0.24)	-0.0008 (-0.40)	-0.0008 (-0.43)
EDUCATION	-0.0301 (-1.01)	-0.0358 (-1.20)	-0.0361 (-1.20)	-0.0366 (-1.21)	-0.0379 (-1.26)
SOUTH	-0.0974 (-1.06)	-0.0944 (-1.03)	-0.0950 (-1.03)	-0.1080 (-1.16)	-0.1026 (-1.11)
SMSA	0.0318 (0.61)	0.0384 (0.73)	0.0381 (0.72)	0.0319 (0.60)	0.0377 (0.71)
UNION	0.3101 (7.17)**	0.3076 (7.13)**	0.3078 (7.12)**	0.3085 (7.12)**	0.3096 (7.18)**
NEVER SERVED	-0.0696 (-0.68)	-0.0540 (-0.53)	-0.0534 (-0.52)	-0.0612 (-0.59)	-0.3211 (-2.68)**
ATTRITER	-0.0211 (-0.25)	0.0097 (0.12)	0.0101 (0.12)	0.0102 (0.12)	-0.1220 (-1.33)
TENURE	0.0027 (3.82)**	0.0023 (3.22)**	0.0023 (3.22)**	0.0023 (3.19)**	0.0021 (3.00)**
WKS WORK	0.0001 (0.28)	0.0001 (0.12)	0.0000[f] (0.12)	0.0001 (0.16)	
WKS NONWORK	-0.0012 (-2.21)**	-0.0011 (-2.08)**	-0.0011 (-2.07)**	-0.0011 (-2.06)**	
LAMBDA	-0.8586 (-1.97)**	-0.8616 (-1.98)**	-0.8634 (-1.98)**	-0.9178 (-2.09)**	-0.9249 (-2.12)**
HEALTH	0.4267 (1.71)*	0.4271 (1.71)*	0.4286 (1.71)*	0.4710 (1.87)*	0.4785 (1.91)*
EMPLOYERS[b]		-0.0149 (-2.04)**	-0.0144 (-1.59)	0.0012 (0.05)	0.0036 (0.17)
OCCUPATIONS[c]			-0.0012 (-0.09)	-0.0573 (-1.26)	-0.0571 (-1.26)
EMPLOY SQ				-0.0014 (-0.90)	-0.0014 (-0.93)
OCC SQ				0.0065 (1.27)	0.0061 (1.18)
WKS WORK (REV)[d]					0.0003 (0.59)
WKS NONWORK (REV)[e]					-0.0012 (-2.38)**
INTERCEPT	7.0506 (10.38)**	7.2057 (10.58)**	7.2130 (10.51)**	7.3422 (10.59)**	7.5850 (11.02)**
$\bar{R}^2$	0.198	0.203	0.202	0.202	0.209
Sample cases	498	498	498	498	498

Table 8A.15 (continued)

[a] White male youth at work as civilian wage and salary earners in survey week 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Number of different civilian jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[c] Number of different kinds of civilian work in regular jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[d] Excludes weeks of military service for youth who previously served.

[e] Includes weeks of military service for youth who previously served.

[f] Less than 0.0005.

* Significant at $\alpha = 0.05$, one-tail test.
**Significant at $\alpha = 0.025$, one-tail test.

Table 8A.16 (ln) Wage Regressions, Black Male Youth: 1983[a]

(t-ratios in parentheses)

Characteristic	Specification				
	1	2	3	4	5
AFQT	0.0052	0.0051	0.0051	0.0050	0.0054
	(2.44)**	(2.41)**	(2.41)**	(2.34)**	(2.58)**
EDUCATION	-0.0022	0.0021	0.0018	0.0063	0.0154
	(-0.05)	(0.04)	(0.04)	(0.13)	(0.31)
SOUTH	-0.0488	-0.0561	-0.0562	-0.0377	-0.0060
	(-0.56)	(-0.64)	(-0.64)	(-0.42)	(-0.07)
SMSA	0.1734	0.1750	0.1747	0.2043	0.2600
	(1.63)	(1.64)*	(1.64)*	(1.89)*	(2.41)**
UNION	0.1652	0.1793	0.1787	0.1813	0.1578
	(2.30)**	(2.45)**	(2.43)**	(2.47)**	(2.15)**
NEVER SERVED	0.1898	0.1648	0.1703	0.1397	-0.3707
	(1.77)*	(1.49)	(1.50)	(1.22)	(-2.43)**
ATTRITER	0.1504	0.1236	0.1208	0.1262	-0.0993
	(1.03)	(0.83)	(0.81)	(0.84)	(-0.62)
TENURE	-0.0000[f]	0.0006	0.0006	0.0006	0.0005
	(-0.01)	(0.39)	(0.39)	(0.42)	(0.32)
WKS WORK	0.0018	0.0018	0.0017	0.0019	
	(2.15)**	(2.15)**	(2.08)**	(2.26)**	
WKS NONWORK	-0.0010	-0.0010	-0.0011	-0.0008	
	(-1.10)	(-1.07)**	(-1.09)	(-0.86)	
LAMBDA	-0.1794	-0.1817	-0.1793	-0.1350	0.0119
	(-0.60)	(-0.61)	(-0.60)	(-0.45)	(0.04)
HEALTH	0.0764	0.0554	0.0552	0.0290	-0.0587
	(0.25)	(0.18)	(0.18)	(0.09)	(-0.19)
EMPLOYERS[b]		0.0148	0.0173	-0.0273	-0.0325
		(0.97)	(0.92)	(-0.56)	(-0.68)
OCCUPATIONS[c]			-0.0057	0.1035	0.0963
			(-0.22)	(1.40)	(1.32)
EMPLOY SQ				0.0042	0.0045
				(1.04)	(1.11)
OCC SQ				-0.0114	-0.0108
				(-1.55)	(-1.48)
WKS WORK (REV)[d]					0.0023
					(2.74)**
WKS NONWORK (REV)[e]					-0.0005
					(-0.54)
INTERCEPT	5.4744	5.3614	5.3824	5.1126	5.2785
	(6.98)**	(6.76)**	(6.71)**	(6.25)**	(6.51)**
$\bar{R}^2$	0.235	0.235	0.230	0.233	0.250
Sample cases	156	156	156	156	156

Table 8A.16 (continued)

[a] Black male youth at work as civilian wage and salary earners in survey week 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Number of different civilian jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[c] Number of different kinds of civilian work in regular jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[d] Excludes weeks of military service for youth who previously served.

[e] Includes weeks of military service for youth who previously served.

[f] Less than 0.0005.

* Significant at $\alpha = 0.05$, one-tail test.
**Significant at $\alpha = 0.025$, one-tail test.

Table 8A.17 (ln) Wage Regressions, Female Youth: 1983[a]

(t-ratios in parentheses)

Characteristic	Specification 1	2	3	4	5
AFQT	0.0064 (4.43)**	0.0063 (4.38)**	0.0064 (4.42)**	0.0064 (4.42)**	0.0063 (4.35)**
EDUCATION	0.0424 (1.45)	0.0403 (1.37)	0.0394 (1.33)	0.0392 (1.33)	0.0388 (1.31)
SOUTH	-0.0216 (-0.55)	-0.0240 (-0.60)	-0.0217 (-0.54)	-0.0205 (-0.51)	-0.0231 (-0.58)
HISPANIC	0.0904 (1.91)*	0.0887 (1.86)*	0.0893 (1.88)*	0.0889 (1.87)*	0.0855 (1.80)*
BLACK	0.1207 (2.42)**	0.1201 (2.41)**	0.1225 (2.45)**	0.1213 (2.42)**	0.1194 (2.38)**
SMSA	0.0740 (2.13)**	0.0761 (2.18)**	0.0779 (2.23)**	0.0780 (2.23)**	0.0811 (2.32)**
UNION	0.1549 (3.80)**	0.1547 (3.79)**	0.1528 (3.74)**	0.1540 (3.76)**	0.1550 (3.78)**
NEVER SERVED	0.2302 (3.44)**	0.2362 (3.50)**	0.2513 (3.64)**	0.2478 (3.57)**	-0.0349 (-0.44)
ATTRITER	0.0809 (0.92)	0.0848 (0.97)	0.0962 (1.09)	0.0914 (1.03)	-0.0560 (-0.62)
TENURE	0.0022 (3.72)**	0.0021 (3.41)**	0.0021 (3.38)**	0.0021 (3.37)**	0.0021 (3.35)**
WKS WORK	0.0002 (0.65)	0.0002 (0.59)	0.0002 (0.64)	0.0002 (0.64)	
WKS NONWORK	-0.0014 (-3.54)**	-0.0014 (-3.57)**	-0.0014 (-3.65)**	-0.0014 (-3.62)**	
LAMBDA	0.2008 (1.35)	0.1862 (1.24)	0.1861 (1.24)	0.1898 (1.26)	0.1799 (1.20)
HEALTH	-0.3349 (-3.47)**	-0.3253 (-3.34)**	-0.3255 (-3.34)**	-0.3277 (-3.36)**	0.3241 (-3.32)*
EMPLOYERS[b]		-0.0045 (-0.70)	-0.0002 (-0.03)	-0.0024 (-0.13)	-0.0027 (-0.15)
OCCUPATIONS[c]			-0.0114 (-1.02)	0.0082 (0.22)	0.0087 (0.24)
EMPLOY SQ				0.0002 (0.17)	0.0002 (0.17)
OCC SQ				-0.0025 (-0.56)	-0.0024 (-0.55)
WKS WORK (REV)[d]					0.0003 (0.77)
WKS NONWORK (REV)[e]					-0.0013 (-3.46)**
INTERCEPT	4.7173 (9.37)**	4.7770 (9.35)**	4.7920 (9.38)**	4.7639 (9.25)**	5.0467 (9.75)**
$\bar{R}^2$	0.198	0.197	0.197	0.195	0.194
Sample cases	703	703	703	703	703

Table 8A.17 (continued)

[a] Female youth at work as civilian wage and salary earners in survey week 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Number of different civilian jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[c] Number of different kinds of civilian work in regular jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[d] Excludes weeks of military service for youth who previously served.

[e] Includes weeks of military service for youth who previously served.

* Significant at $\alpha = 0.05$, one-tail test.
**Significant at $\alpha = 0.025$, one-tail test.

Table 8A.18 Means and Standard Deviations for (Ln) Wage Equations, by Sex and Race: 1983[a]

(Standard deviations in parentheses)

Characteristics	Male			Female
	White	Black	Total	
AFQT	74.81	49.20	67.93	70.32
	(16.47)	(18.14)	(20.15)	(17.38)
EDUCATION	12.02	11.94	12.00	12.23
	(0.77)	(0.79)	(0.79)	(0.78)
SOUTH	0.31	0.57	0.36	0.39
	(0.46)	(0.50)	(0.48)	(0.49)
HISPANIC			0.13	0.13
			(0.33)	(0.34)
BLACK			0.21	0.19
			(0.41)	(0.39)
SMSA	0.70	0.78	0.73	0.72
	(0.46)	(0.41)	(0.44)	(0.45)
UNION	0.23	0.35	0.27	0.15
	(0.42)	(0.48)	(0.44)	(0.36)
NEVER SERVED	0.74	0.74	0.76	0.88
	(0.44)	(0.44)	(0.43)	(0.32)
ATTRITER	0.07	0.10	0.07	0.06
	(0.25)	(0.30)	(0.25)	(0.23)
TENURE	48.84	46.46	48.03	47.76
	(27.93)	(25.91)	(27.44)	(26.34)
WKS WORKED	247.98	231.52	242.27	219.17
	(60.37)	(64.58)	(62.33)	(67.49)
WKS NONWORK	39.99	56.44	45.30	65.10
	(45.54)	(53.61)	(49.25)	(60.41)
LAMBDA	0.40	0.53	0.43	0.57
	(0.16)	(0.21)	(0.17)	(0.26)
HEALTH	0.02	0.02	0.02	0.05
	(0.14)	(0.14)	(0.14)	(0.22)
EMPLOYERS[b]	4.02	3.72	4.05	3.75
	(2.71)	(2.57)	(2.75)	(2.48)
OCCUPATIONS[c]	3.86	3.89	3.88	3.61
	(1.76)	(1.85)	(1.76)	(1.66)
EMPLOYER SQ	23.50	20.43	23.90	20.19
	(35.23)	(28.95)	(35.61)	(30.05)
OCC SQ	18.00	18.53	18.17	15.83
	(15.56)	(17.66)	(15.89)	(13.59)
WKS WORKED (REV)[d]	207.74	193.06	205.07	204.46
	(84.67)	(83.37)	(83.68)	(78.38)
WKS NONWORK (REV)[e]	80.22	94.89	82.50	79.82
	(85.14)	(83.37)	(83.76)	(73.92)
WAGE (cents)	701.75	597.86	680.53	513.16
	(327.50)	(318.50)	(324.83)	(198.29)
(LN) WAGE	6.45	6.28	6.42	6.16
	(0.45)	(0.47)	(0.45)	(0.43)
Sample cases	498	156	748	703

Table 8A.18 (continued)

[a] Youth at work experience as wage and salary earners in survey week 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Number of different civilian jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[c] Number of different kinds of civilian work in regular jobs held between the date left school and the date of interview in 1983. Excludes all post-school civilian work experience prior to January 1, 1978.

[d] Excludes weeks of military service for youth who previously served.

[e] Includes weeks of military service for youth who previously served.

Table 8A.19 Employment Probability Equations, by Reference Period, Sex and Race: 1983

(asymptotic t-ratios in parentheses)

Characteristics	Male			Female
	White	Black	Total	
1983 Survey week experience^a				
AFQT	0.0080	0.0043	0.0073	0.0127
	(2.53)**	(0.88)	(3.01)**	(5.09)**
EDUCATION	0.0804	0.1000	0.1096	0.2776
	(1.18)	(1.04)	(2.15)**	(5.72)**
SOUTH	0.4535	0.3213	0.4109	0.3049
	(3.69)**	(1.78)*	(4.37)**	(3.81)**
SMSA	0.1862	0.4469	0.2359	0.0906
	(1.68)*	(2.22)**	(2.59)**	(1.09)
HISPANIC			0.1253	0.1231
			(0.92)	(1.01)
BLACK			-0.2082	-0.3016
			(-1.80)*	(-2.86)**
NEVER SERVED	0.4548	-0.2974	0.3018	0.2829
	(3.58)**	(-1.07)	(2.74)**	(1.75)*
ATTRITER	-0.0292	-0.4326	-0.0169	0.0042
	(-0.14)	(-1.20)	(-0.10)	(0.02)
HEALTH	-0.8833	-0.8597	-0.8578	-0.7501
	(-3.73)**	(-2.45)**	(-4.63)**	(-6.14)**
MARRIED	0.1723	0.5527	0.2675	-0.3580
	(1.63)	(2.47)**	(3.00)**	(-4.74)**
INTERCEPT	-1.4562	-1.3067	-1.7154	-4.0502
	(-1.86)*	(-1.12)	(-2.91)**	(-7.18)**
Sample cases	751	272	1161	1335
-2 Ln L	56.53*	27.29**	94.29*	210.45**
1982-1983 experience^b				
AFQT	0.0128	0.0080	0.0100	0.0144
	(2.95)**	(1.34)	(3.09)**	(5.36)**
EDUCATION	-0.0828	0.0508	0.0060	0.3197
	(-0.88)	(0.44)	(0.09)	(6.10)**
SOUTH	0.3364	0.7339	0.4701	0.2502
	(1.82)*	(3.21)**	(3.51)**	(2.83)**
SMSA	0.0440	0.4597	0.1231	0.1859
	(0.28)	(1.85)*	(0.99)	(2.06)**
HISPANIC			0.0902	0.0746
			(0.47)	(0.56)
BLACK			-0.3572	-0.2697
			(-2.42)**	(-2.41)**
NEVER SERVED	0.0803	0.1220	0.0444	0.3994
	(0.41)	(0.37)	(0.27)	(2.29)**

Table 8A.19 (continued)

Characteristics	Male			Female
	White	Black	Total	
ATTRITER	-0.4231	-0.1275	-0.3440	0.2181
	(-1.55)	(-0.30)	(-1.54)	(0.93)
HEALTH	-0.6243	-0.7249	-0.6750	-0.4756
	(-2.23)**	(-2.09)**	(-3.33)	(-3.95)**
MARRIED	0.1368	0.5276	0.2706	-0.3002
	(0.89)	(1.74)*	(2.10)**	(-3.60)**
INTERCEPT	-1.4653	-0.7738	0.4972	-4.3292
	(1.35)	(-0.56)	(0.64)	(-7.05)
Sample cases	751	272	1161	1335
-2 Ln L	23.22**	21.05**	64.18**	182.05**

[a] Youth at work as civilian wage and salary earners in survey week 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Youth with civilian work experience as wage and salary earners between 1982 and 1983 interview dates, and who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

* Significant at $\alpha = 0.05$, one-tail test.
**Significant at $\alpha = 0.025$, one-tail test.

Table 8A.20 Means and Standard Deviations for Employment Probability Equations, by Reference Periods, Sex and Race: 1983

(standard deviations in parentheses)

Characteristics	Male			Female
	White	Black	Total	
AFQT	73.35	47.53	65.71	65.98
	(17.65)	(18.53)	(21.20)	(19.53)
EDUCATION	11.96	11.78	11.90	12.03
	(0.82)	(0.90)	(0.86)	(0.88)
SOUTH	0.27	0.57	0.34	0.36
	(0.45)	(0.50)	(0.47)	(0.48)
SMSA	0.68	0.74	0.71	0.71
	(0.47)	(0.44)	(0.45)	(0.45)
HISPANIC			0.12	0.12
			(0.32)	(0.32)
BLACK			0.23	0.23
			(0.42)	(0.42)
NEVER SERVED	0.72	0.78	0.75	0.89
	(0.45)	(0.42)	(0.43)	(0.32)
ATTRITER	0.08	0.10	0.08	0.06
	(0.27)	(0.29)	(0.27)	(0.23)
HEALTH	0.04	0.06	0.04	0.10
	(0.20)	(0.23)	(0.21)	(0.31)
MARRIED	0.40	0.21	0.35	0.49
	(0.49)	(0.41)	(0.48)	(0.50)
EMPLOY PROB, SW83	0.74	0.64	0.72	0.59
	(0.44)	(0.48)	(0.45)	(0.49)
EMPLOY PROB, 1982-83	0.93	0.84	0.91	0.76
	(0.25)	(0.37)	(0.28)	(0.43)
Sample cases	751	272	1161	1335

Table 8A.21 Collective Bargaining Coverage and Mean Tenure in Survey Week (Last) Job, by Civilian and Military Status, Sex, and Race: 1983[a]

	Male									
	White		Black		Total[d]		Female		Total	
Characteristic	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean	Sample cases	Mean
					Tenure (weeks)					
Never served	514	51.2	172	44.2	804	50.2	801	48.0	1605	49.2
Previously served[b]	225	33.7	58	37.2	304	34.3	128	27.9	432	33.8
Veteran[c]	170	32.9	35	39.9	220	34.2	75	31.8	295	34.1
Attriter	55	34.7	23	e	84	34.4	53	26.1	137	33.5
Total	742	49.5	230	43.1	1111	48.6	931	47.8	2042	48.2
					Collective bargaining coverage (percent)					
Never served	544	24.6	186	29.8	857	25.3	936	12.8	1793	19.1
Previously served[b]	254	21.7	62	33.2	340	23.6	156	23.4	496	23.6
Veteran[c]	190	17.9	37	25.3	245	19.6	86	13.3	331	19.4
Attriter	61	23.4	25	41.9	92	26.4	68	28.5	160	26.6
Total	798	24.3	248	30.4	1197	25.1	1092	12.9	2289	19.4

[a] Civilian youth interviewed in 1983 who left school between 1976 and 1978 with 10 to 14 years of schooling completed.

[b] Includes former enlistees whose completion status in first tour is not known.

[c] Includes former enlistees who were within two months of completing their initial tour at time of discharge.

[d] Includes Hispanic male youth.

[e] Less than 25 sample cases.